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Philosophy of Science

VOL. 5 April, 1938 NO. 2

Editorial

Where Is Philosophy of Science?

THE time has come for the editor to report on the progress of his journal. *Philosophy of Science* is in its fifth year. For four years we have experimented with papers of every variety, with subjects of both broad and narrow scope, and with styles designed primarily to instruct or primarily to entertain. We have been accused of sponsoring any one of a dozen movements as well as their opposites. We have been both blamed and praised for opinions not our own. We have been urged to swing right or to swing left, to abandon science or to abandon philosophy, to join in certain aversions or in certain enthusiasms of the day, and to do all those things promptly and full-heartedly on pain of excommunication, i.e. cancellation of subscription.

An example is needed. Some men of faint heart were greatly alarmed by the inclusion of articles which contained symbolic material. In spite of the fact that no more than 10 per cent of the space in any one year was used for that purpose, several stated with conviction that the journal had fallen into the hands of professional logicians who filled it *completely* or almost completely with cryptic symbols. On the other hand several distinguished founders of the *Journal of Symbolic Logic* found the 10 per cent of our space all too little, and actually felt they needed a special journal to obtain adequate pasture.

Another example. Several letters on the part of the editor failed to convince a noted American philosopher that we were not exclusively a government by logical positivists, a mere adjunct

of the Vienna Circle. The correspondence took place when four dialectical materialists, two convinced metaphysicians, a vitalist, and a mild defender of mysticism had had their say in the pages of the journal. Only two members of the Vienna Circle and a sympathizer had represented the positivist voices. Were those voices too loud, or was the eminent philosopher too impatient with differences of opinion?

* * *

In the distribution of attention to the individual sciences physics proved to be the favorite. This preponderance, too, was exaggerated by some commentators to an undue extent. The allusions to physics occurred principally and naturally in articles on methodology and on such topics as causality, time and space. There were times, however, when psychology and biology took the lead. The social sciences and the linguistic aspects of science have also been well represented. Historical and philological material has appeared. Now and then the humorous department, *A New Budget of Paradoxes*, took up the perennial battle with the cranks who prove Einstein and Newton naively wrong and persist in squaring the circle.

* * *

Allowing for a cancellation of prejudices, the editor still has the ever-pressing responsibility of sustaining the positive, liberal program announced in the first issue of the journal, a program satisfactory to a many-hued Editorial Board and to an equally colorful Advisory Board, a program of *free* discussion or of an open forum conducted in the *research spirit* of a united front against blindness.

We invite readers to present their criticisms freely but fairly, to request specifically the kind of material for which they feel a need, and to consider carefully the advisability of taking an interest in subjects away from their specialization even at the cost of some effort.

We invite contributors, who have been hanging back on account of some notion that we are sectarian, to present *their* views, and to join battle with those who have spoken first. We have

published material with which the entire editorial board was in disagreement. We felt that the authors were sincere and represented prevalent opinions. Subsequent issues contained what we thought were refutations. The debates, we felt, should have gone on and should continue to go on.

* * *

The following resolutions for the fifth year are announced unreservedly:

1. We shall discountenance unnecessary "difficult writing".
2. We shall censor unnecessary technicalities.
3. We shall encourage clarity and brevity, delicately spiced with wit.
4. We shall encourage papers which give the *factual* and *historical* background of their discussion.
5. We shall allow for a certain amount of papers devoted to bringing the general reader up-to-date on the progress of the sciences, e.g. "the present status of genetics", or "present problems in astronomy" and the like.
6. We shall allow for a certain number of papers devoted to simple expositions of specialties that people wish to know better, e.g. "what is group theory?", or "what is phyloanalysis?", or "what is dynamic cytology?"

In short, we shall try to construct on the basis of several years' experience a more readable, a more comprehensive, and yet, a more penetrating journal of free research in the philosophy of science.

W. M. M.

Operational Analysis

BY

P. W. BRIDGMAN

IN THE October 1937 number of *Philosophy of Science* Lindsay has made certain criticisms of the adequacy of the "operational method" of analyzing and giving meaning to the concepts of physics, documenting his criticisms chiefly from my own writings. In these criticisms he has made statements as to the method which I would by no means accept. This is not characteristic of his paper only, for I have seldom indeed seen a printed discussion of the method which I would accept as being an adequate or sometimes even fair representation of what I understand by it. It will perhaps pay therefore if I attempt to state what I conceive it to be all about, particularly since I have never attempted such a comprehensive statement and since my own ideas on the subject have been developing since I first wrote in the Logic of Modern Physics. For one reason I have hesitated to do this, for fear of seeming to subscribe to the not uncommon idea that we are dealing with some elaborate and profound new theory of the nature of knowledge or of meaning. I believe that I myself have never talked of "operationalism" or "operationism", but I have a distaste for these grandiloquent words which imply something more philosophic and esoteric than the simple thing that I see. What we are here concerned with is an observation and description of methods which at least some physicists had already, perhaps unconsciously, adopted and found successful—the practise of the

methods already existed. What I have attempted is to analyze these successful methods, not to set up a philosophical system and a theory of the properties that any method *must* have if it hopes to be successful. Since I was concerned with a technique already extant, my principal method of getting others to see what the technique involved has been to exhibit examples of the technique in action, rather than to attempt any exhaustive characterization of the technique itself.

It is probably pertinent to mention the personal background of my own work. My discussion of the operational technique came only after at least ten years of more or less continuous pondering in the effort to see what was really happening in branches of physics that puzzled me. In this way I had thought through the situation in dimensional analysis, a comparatively simple field, but in which nevertheless there was a great deal of mysticism, and afterwards I had tried to see what is really involved in electromagnetic field theory, relativity theory, and those parts of quantum theory which had been developed by 1926. It was only then, after this background of my own attempts at analysis and of reading the analyses of others, that I began to see what successful analysis consists in, so that my ideas about the operational technique resulted from observation and practise of analysis. The analysis which was the object of my observation was an analysis made by physicists at a certain epoch in history, with the background of assumption and presupposition prevalent at that epoch, and with the unexpressed purposes inherent in the scientific activities of the epoch.

The analysis that was the object of observation was made for the general purpose of getting better understanding, and this in turn was seen to demand the maximum awareness of every conceivable thing that could be found in the situation. In analyzing one seeks to become aware of all the details of the physical manipulations or other happenings, and also of the details of his own thinking about these happenings in attempting to reduce them to understandability. It is a simple result of observation that the subject matter of all these attempts at understanding is activity of one sort or another, either in the perception and recognition

by us of sense impressions, or in the performance by us of deliberate physical manipulation or of deliberate thought. In any event the subject of analysis is activity, and one object of analysis is to discover activities of which we had not previously been aware, and to find relationships between activities. The fact that we are concerned only with activities is reflected in a broad sense in which this matter of operations can be understood, as opposed to a more important narrow sense to be discussed presently. In this broad sense any analysis of activity is an analysis of operations. This is tautological; operations and activity here mean the same thing. The word "operation" accents the fact that the activity is usually directed activity. I report as a matter of observation that I have never come across a situation in physics, or in other fields either for that matter, in which a complete analysis of activity would not have satisfied me. We need not discuss whether there may possibly be other things to analyze, but if there are, they have not proved necessary for my purpose. Consider in particular the question of meanings, which are evidently of the greatest concern for any analytical enterprise. To know the meaning of a term used by me it is evident, I think, that I must know the conditions under which I would use the term, and conversely, to know the meaning of a term which my fellow uses, I must be able to reconstruct the conditions which compelled the use of the term by him. Analysis of the conditions attendant on the use of the term is an analysis of activity of one sort or another, or in other words, an analysis of operations. From this point of view, *meanings are operational.*

Notice that we are not attempting to set up any theory of meaning, and do not maintain that meaning involves nothing more than operations. We are dealing with a necessary as distinguished from a sufficient characterization, in the sense of the mathematician. We are saying that unless one knows the operations one does not know the meaning. It is perhaps possible to see other elements in the complex covered by "meaning". One may perhaps be concerned with trying to account for why it is that in a given situation one should want to separate out certain particular aspects to accent by meaning. Again experience and

observation determine our course. In the sorts of situation that have been of interest to me, and I judge to other physicists also, it is not necessary to be concerned with other than operational aspects of meaning. From this point of view the statement "meanings are operational" comes to be used carelessly as a *sufficient* description of the whole situation. In fact, I myself have doubtless gone further in some of my printed statements than would be justified when the statement is taken out of context. For instance, my statement on page 5 of the Logic of Modern Physics that meanings are *synonymous with operations*, was obviously going too far when taken out of context; it was in any event going farther than necessary. My own dictum is applicable to this statement of mine, namely that what a man means by a term is to be found by observing what he does with it, not by what he says about it.

The statement "meanings are operational" is so broad and general as to be tautological, and furthermore to be of little value. Operational analysis does not begin to be important until we impose certain restrictions. The situations that we analyze are practically always situations in which we are interested in accomplishing certain purposes; at the very least it is our purpose to understand. Experience has shown that certain sorts of operation are no good for accomplishing certain purposes, but that we can hope to encompass our end only by the use of restricted operations. Some of the necessary restrictions on the operations are so obvious and intuitive that we unconsciously limit the operations which we will permit ourselves in definite situations, so that if analysis discloses that the operations which we actually use in giving our meanings are drawn from outside the permitted repertory, we describe the situation by saying that the term is meaningless "because the operations do not exist." Obviously this is a narrow and a technical use of "meaningless", because from the broad point of view every term has some sort of meaning and the performance of some sort of operation was antecedent to its use. It is profitable nevertheless to permit ourselves to talk about "meaningless" terms in the narrow sense if the preconditions to which all profitable operations are subject are so

intuitive and so universally accepted as to form an almost unconscious part of the background of the public using the term. Physicists of the present day do constitute a homogeneous public of this character; it is in the air that certain sorts of operation are valueless for achieving certain sorts of result. If one wants to know how many planets there are one counts them but does not ask a philosopher what is the perfect number.

It is obvious that a great deal of experience is back of the conviction that certain sorts of operation are not good operations and that others are probably safe, and as one's experience changes one's repertory of permissible operations changes also. The Greek philosophers esteemed highly certain operations which today are beneath contempt. I suppose the intellectual attitude of the present time differs most from that of the time of the Greeks in its position toward experiment and experience. Hundreds of years of attempting to find inside our own heads the necessary pattern of the external world has proved a dismal failure; we have come to feel that we can only take experience as it comes and must try to get our thinking into conformity with it.

Since experience follows no prescribable pattern, an indispensable part of the task of attempting to reduce experience to understandability is to find how to describe or reproduce that experience as accurately as possible. No detail is to be neglected as trivial until the proof has been given that it is trivial, and the only acceptable proof of triviality is an appeal to experience. In describing a "physical" experience (we all have a sufficiently common background so that the meaning of "physical" is sharp enough), observation shows that we recognize certain sorts of activity on our part as being much more definite and getting closer to what we may call the primary experience than other sorts of activity. There are certain sorts of operation that seem to us simple in the sense that when we are told to perform the operation we can go and do it unequivocally, whereas there are other operations which have perhaps just as simple a verbal expression but which nevertheless, the moment that it comes to the actual doing of them, are found to involve imprecisions and ambiguities. Thus if we are told to determine the time at which

some event occurs, we can do it without ambiguity if the event occurs here beside a clock. But if the event occurs on Mars we have complications arising from corrections for the velocity of light and the distance of Mars, if we elect to determine the time on our own clock, and also ambiguities when we ask whether the time is more properly determined from our own clock or from a clock on Mars, whose rate and setting we will have to check by some sophisticated procedure. Observation shows that ambiguity is latent in any operation defined in terms of its properties, because of the inherent experimental error with which properties can be established. It seems evident enough that we should seek to reduce our descriptions of experience to unambiguous kinds of operation, and that the terms or concepts in which we frame our descriptions should be specified by such operations. Otherwise we cannot uniquely reproduce the original situation, and our attempt at description has failed. Hence arises the demand that the concepts or terms used in the description of experience be framed in terms of operations which can be unequivocally performed.

We have here no esoteric theory of the ultimate nature of concepts, nor a philosophical championing of the primacy of the "operation". We have merely a pragmatic matter, namely that we have observed after much experience that if we want to do certain kinds of things with our concepts, our concepts had better be constructed in certain ways. In fact one can see that the situation here is no different from what we always find when we push our analysis to the limit; operations are not ultimately sharp or irreducible any more than any other sort of creature. We always run into a haze eventually, and all our concepts are describable only in spiralling approximation. We can, if we want, analyze the operation which our physical experience allows us to accept for certain purposes as unanalyzable. We find that no operation is ultimately simple. We can always ask ourselves how we are sure that we are correctly performing the operation that we assume, or how we shall describe the operation to our fellow who has not had our experience. We shall probably find that in attempting to get our fellow to acquire intuitive command of our opera-

tion we have described our operation in terms of its properties, something that we must avoid wherever possible. Experience shows that the things we do can be arranged in hierarchies of complication and certainty. We analyze as far back as necessary for our purposes, and frame our concepts in terms of operations which experience suggests are simple enough for our purposes. Modern physics finds that certain operations are simple enough for its purposes while others are too complex. For the geometrical description of the objects of ordinary experience and for all the geometrical descriptions of relativity theory, the operation of determining a *coincidence* is accepted as an operation intuitively performable, which it is not necessary to analyze further. We endeavor as far as possible to build up other operations from this simple operation. Some day the discovery of new physical facts may force us to analyze the coincidence operation, but at present we do not see the necessity for it, and in fact do not see how the analysis can be made, and feel a certain security in any analysis which is reduced to this fundamental operation. In particular, we define the length of a body in terms of operations involving making coincidences of the ends of a meter stick moved successively along the body. At first every step of the procedure by which the length is measured must be uniquely specified if we are to be sure that our result is unique. We may not leave out as unessential a single detail of procedure until experience has shown that that detail makes no difference. For example, a length specified by moving the meter stick from one positon to the next maintaining itself always parallel to itself must at first be distinguished from a length determined by rotating the stick through 180° for each successive position. Our present day attitude is that it is not *safe* to assume that the results of the two procedures are the same until we have proved them the same by experience. The Greek perhaps could convince himself in his head that the two procedures *must* give the same result; we have lost his confidence in our ability to foresee the results of experience, and find it almost impossible to recapture the feeling of the Greek that he had a right to expect that he could do this sort of thing. Of course again we ultimately encounter haze; for no operation can

be uniquely specified in all its details. We ultimately have to assume from sheer weariness, if nothing else, that it is not necessary to carry our specifications beyond a certain degree of refinement; the place where we draw the line is again dictated by experience. But experience has sufficiently shown that refinements which at one time were unthinkingly assumed to be without importance are actually important when the range of our experience is extended or the accuracy of our measurements is improved. Thus at one time no one thought of the necessity of distinguishing between a tactual length or an optical length. Relativity theory now shows the necessity for distinction.

This matter of different operations which give very nearly or perhaps the identical result brings up one of the strongest objections that have been made to the view of the relation of operations and concepts just described. Thus at the end of the last paragraph we spoke of a "tactual length" and an "optical length". Many people would say that these expressions are merely paraphrases for "*length* measured by a tactual procedure" and "*length* measured by an optical procedure", and would understand by the concept "length" whatever it is that is denoted by the common word "length" in the two expressions. This is a perfectly possible point of view, but is to a large extent verbal, for every one must agree that experience shows that there are a great many cases in which obviously different operations yield the same result, and that in practise we call by a single name the common result of all the different equivalent operations. Furthermore, I think everyone would agree that for a great many practical purposes this is a most useful thing to do, and that one would be merely going out of his way to invent and use different names for different equivalent operations. But on the other hand, when one's purposes are not immediate practical applications, but when one is trying to analyze and to understand, one must seek to recover the complexity of the primitive situation. If one has shown that "optical length" and "tactual length" are equivalent, then for the purposes of analysis this is something to be remembered, perhaps by the invention of a special nomenclature like length_1 and length_2, rather than something to be deliberately

forgotten by the adoption of a single term. The reason that this is something to be remembered rather than to be forgotten is that it is not *safe* to forget. The equivalence of two operations is established by experiment, and we must always adopt the attitude that the results of such an experimental proof may be subject to revision when the range or accuracy of our experience is increased.

The feeling that it is not *safe* to forget distinguishably different procedures is in the atmosphere of present day physics; it is the result of a wide experience and is not shared by anyone like the Greek whose experience is so narrow that he can still think that all experience must conform to the pattern of something that he finds in his head. All that anyone can maintain here is that for purposes of analysis and understanding it is *better* at first to employ concepts deliberately framed so as to keep in view recognizable differences of procedure. Of course later, perhaps for the purposes of some special theory, one may find it convenient to fuse the results of recognizably different operations under a single name, but it is never safe to *completely* forget what one has done.

Some people are made uncomfortable by insistence on the operational aspect of concepts because they want to make "concept" cover somehow the fact that the results of the operations have been found useful, and also the precise way in which they prove useful. This again is largely a verbal matter and a matter of taste. Perhaps "concept" can be given this extended connotation, but one certainly enormously complicates the situation by trying to do so and furthermore does not escape uncertainties arising from experimental error, for the precise use which one will make of a concept in dealing with experience may be expected to change as the range of experience changes.

We have not as yet made any explicit limitations on the sort of operations that we should employ except that they should be unambiguous, but it has happened that our examples have involved simple "physical" manipulations such as can be performed in the laboratory, as, for example, in our description of the meaning of length. This has led to what I regard as the most wide spread misconception with regard to the operational technique. It is

often stated that the operational point of view demands that all the concepts of physics, for example, find their meaning in terms only of physical operations performed in the laboratory. Thus Lindsay states in the second paragraph of his paper: "The essential meaning of operationalism in physics is that physical concepts should be defined in terms of actual physical operations". It is true that he later qualifies this slightly, but he evidently regards the qualification as unimportant. Nothing could be further from my own attitude, in fact I think examination of my writings will show that "mental" operations have been often mentioned. It seems to me that the most superficial examination of what we do in any situation, even a situation which we might perhaps describe as predominantly "physical", shows at once that "mental" operations are involved, and further that no sharp distinction is possible between "physical" and "mental" operations. For instance, we measure the length of an object by counting the number of times that a meter stick can be brought into coincidence in a special way with the object. Is the operation of counting a "mental" or a "physical" operation? My own use of language would make the counting operation "mental", yet this is an integral part of many "physical" procedures.

By an extension of this same point of view it is often maintained (Lindsay does it) that the operational point of view demands that the physicist use no constructs in his theorizing, or in general in his efforts to understand, which do not find their meaning in exclusively physical operations. This it seems to me to be a restriction for which the only justification could be that experience had shown it to be a useful restriction. But the most superficial examination of what physicists do when they theorize shows that as a matter of fact they do find it very useful to employ all sorts of constructs not defined by purely physical operations. Most of these non-physical operations are the operations of mathematics or logic; it is particularly obvious in the case of modern wave mechanics that many of the constructs are of this sort. "Paper and pencil" operations is perhaps a suggestive name for many such operations. The variety of such possible "paper and pencil" operations is doubtless greater than the variety of conventional

operations of the laboratory; the enormous wealth of such operations makes them a most fertile field for the invention of intermediate constructs. Many of the "paper and pencil" models constructed in this way are of great value. For example, my analysis of the concept of probability showed that it was to a very large extent a "paper and pencil" concept. Lindsay seems to feel that I implied that therefore it was not a useful or permissible concept, but such was by no means my intention. Of course when the ultimate object of the theory is the description of a concrete physical situation, we must demand that the ultimate outcome of the theory be expressible in terms of operations applicable in the concrete physical situation, but I can see no reason why one should not allow himself any latitude whatever in his intermediate constructions.

Whatever the nature of the concept, whether it involves the "physical" operations of the laboratory or the "paper and pencil" operations of one or another kind of construct, we must demand that the operations be such that they can be unambiguously and straightforwardly performed. This is a practical matter; no operation can be specified with complete sharpness "unambiguously and straightforwardly", but observation of what we do in meeting the situations of experience shows that as a matter of fact there are certain operations about whose performance we have no hesitation. This is true of the operations of mathematics no less than of the operations of the laboratory. One such operation, for example, is that of increasing any given integer by unity. I believe that no mathematician has the slightest question but that this operation can always be performed. There are other operations in mathematics which cannot be performed so unambiguously, such as operations involving infinity; these operations are often specified by their properties. In mathematics much the same situation arises that arises in physics; in foundation studies, in which one wants to secure the maximum awareness of what one is doing and the maximum security that he is not involving himself in contradiction, one would do well to use only concepts whose meaning is found in such unambiguously performable operations. For in this way, just as in physics, the

description of a situation in mathematics reduces to the description of an actual experience, namely the performed operations, and actual experience is not self-contradictory. Even this procedure cannot give absolute security, for there is no method of guaranteeing the future of the operations we now accept. In particular one can never be sure that at some time in the future he will not think of something which had not previously occurred to him which will make operations unsatisfactory in which no flaw is at present suspected. Even the operation of adding unity to an integer is subject to this limitation. It is merely that this technique is the best which we have.

I think there are at least two reasons why an operational analysis is important. The first is that we usually have preconceived ideas about the operational situation, picked up perhaps by imitating the assumptions which our fellows make in their use of concepts, and the analysis often shows these preconceptions to be incorrect, so that we discover that we had been trying to do things that we had not realized. The second reason is that having found what the situation is we are then in a position to utilize our experience. Experience shows that certain kinds of operation are good for certain purposes, and if our concepts do not involve the kind of operation that we had supposed, then we revise our opinion of what the concept is good for, and the use which we accordingly make of it.

Verbalizing plays an important part in the thinking of most people and, correspondingly, verbal operations are very extensively employed in thinking. Apparently most of our verbalizing arises from the endeavor to make adequate connection with experience, and accordingly, many of our verbal operations are of the greatest value in dealing with experience. In particular, practise has shown us how to use various blank verbal forms with effectiveness, as, for example: "What shall I do next?", or "Who said that?" etc., etc. Substituting in verbal forms constitutes one sort of verbal operation. The use of such verbal forms may be of great value in analyzing the operational make-up of our concepts, for in this way we can make ourselves aware of the conditions under which we would use a term. We may, for example,

ask ourselves, "Would you say this or that about such and such a term?" By asking ourselves this sort of question not only do we uncover bits of "objective" knowledge of the situation which we may have in the back of our heads, but I think we also discover that we often unthinkingly try to force a concept into such a mold that we *can* say certain things about it. That is, we often make unsuspected verbal demands of our concepts. Suppose, for example, we are trying to make more precise to ourselves what we mean by "life" and "death". Various criteria that might be applied in the laboratory are proposed, but then someone says "Would you say that an organism was dead if it could be brought back to admitted life by the administration of any drug, known or unknown?" I think a great many people have a strong impulse to answer this question in the negative. By making this verbal experiment we discover that we unthinkingly use the concept of "life and death" as if it admitted certain verbal operations. But we also demand that the concept be defined in terms of "objective" operations. Common usage of language seems to show that many people have the primitive feeling that some "objective" criterion of life and death can eventually be found corresponding to the verbal demand, but I think as we grow in experience we lose the feeling that there *must* be some laboratory operation corresponding to the verbal demand. Such a feeling can have only vicious results when we want to achieve the maximum security.

It is thus evident that there may be "good" or "bad" verbal operations. Here, as always, there is no evidence that a *sharp* distinction can be drawn between "good" and "bad". It is, I think, the tacit understanding back of the usage of most terms that the meanings can eventually be reduced to non-verbal operations. In describing the situation, "verbal" may be used in the broad sense, including all sorts of verbalization, or in the narrow sense of "bad" verbalization, that is, a verbalization not adapted to its tacit purpose. A concept which is predominantly verbal only in the "bad" sense is often described as a "verbal" concept without qualification, and the sense in which "verbal" is used often has to be judged from the context.

A great many of our common inherited concepts turn out to be

predominantly verbal in the narrow or bad sense, such as all the old absolutes of physics. "Absolute" length, for example, has actually been defined as "length perceived in the mind of God". Analysis will show I think that "perceived in the mind of God" is almost entirely verbal—the operations which determine its meaning are mainly the operations of saying certain things about it. To the modern physicist it is so obvious that concepts defined only in terms of verbal operations are unsuitable for meeting "objective" situations that he automatically does not consider such operations, so that he would describe the concept of "absolute" length as one for which "no operations exist", and therefore "meaningless." This leads to a certain ambiguity or even self contradiction in talking about the operational technique. From the broad point of view all meanings are operational, and the meaning of "absolute" length is to be found in the corresponding operations, which here turn out to be verbal, whereas from the narrower point of view of the present day physicist the operations corresponding to the concept "do not exist". I think if isolated parts of my writings are juxtaposed, it would be possible to find such contradictions. Thus I have talked about the contrast between what one says about what one does and what one actually does, with the implication that what one says is not a form of doing, whereas in other places it is very definitely recognized that talking or verbalizing is certainly an activity. I think, however, that if one is on the lookout it should always be easy to judge from the context whether the broad or narrow meaning of operation is to be understood.

We have seen that as long as one is concerned only with physical or similar applications the profitable application of the operational technique is so obviously restricted to its narrower aspect that in practise one is inclined to forget the broader aspects. But when one comes to make applications to less precise situations, as to economics or law or in general to situations of predominantly social complexion, one encounters more importantly the verbal aspects and the necessity for adopting the broader point of view. In a book just issued I have discussed social matters with this technique, and I have sometimes felt my hands

somewhat tied by the narrower usage of "operations" to which consistency with my earlier writings would sometimes seem to commit me. However, I do not think that there can be a serious source of misunderstanding, and that the context always makes it plain whether operation is to be understood in the broader sense of anything, mental, manual, or verbal, that can be recognized as activity, or in the narrower sense of activity that is obviously adapted to the tacit purpose.

I think that trial will convince one that no less important results are to be obtained by the application of the operational technique to social questions than to physical questions. For we nearly always have preconceptions as to what our social concepts are good for and the character of the underlying operations, and we use the concepts subject to those preconceptions, whereas analysis will often disclose that the operations which build up the concept are such that we have no right to our expectations. Analysis will often disclose that the only operations which can be found in a concept are verbal operations. There are many cases of open verbal chains; the meaning of a term is to be found in the fact that it can be combined verbally in a certain way with another term, and the meaning of the new term in its verbal combinations with still another, and so on endlessly, with never an exit into something "objective", that we do with our hands or see with our eyes or feel. [Notice that "objective" is not being used in the sense of contrasted to "subjective", but may include certain aspects of the "subjective".] The implication in the usage of a great many of our terms, however, is that ultimately there is emergence into something "objective". I think there can be no question but that we use such words as "justice" or "truth" with the implication of such ultimate objective emergence of meaning. The discovery that such emergence often does not occur, but the only operations are verbal, will I think profoundly modify our practical attitude toward the social situations to which we have been accustomed to apply the concept.

It must not be at once assumed that any concept which is almost completely verbal in this way can be of no value and must be discarded. I think there are many concepts of this character,

most useful as intermediate terms in our thinking, and not to be discarded because of their verbal character, any more than the Ψ function of wave mechanics is to be discarded because its value cannot be assigned at every point of space and time with the instruments of the laboratory. Clean cut classification of terms into those which are purely verbal is not to be expected here anymore than anything clean cut is to be expected anywhere. In particular there is obviously no sharp distinction between "paper and pencil" operations and verbal operations. But there are certainly terms in which the verbal element is much more important than we would have guessed without analysis, and some of these may be of the greatest utility in every day usage. For example, if we analyze what we mean by "the past" we discover that a large part of the meaning is verbal. Most people would say that the past is such a thing that there are "facts" with respect to the past, as for example, it is a "fact" that the sun rose this morning. But if someone says to us "There is no way of proving that the entire universe was not created five minutes ago, every part of it being created in the condition that you think it had reached five minutes ago as the result of age-long evolution" we can find no method of controverting him and are made to see that the operations defining "the past" are more largely verbal than we had supposed. Although we have a concept that is to a certain extent verbal, it is nevertheless useful. This shows incidentally that our verbalisms have no automatic method of ensuring self-consistency, and that we do well to be cautious in our handling of predominantly verbal concepts.

One can envisage here a new discipline, as yet hardly begun, of the detailed analysis of the operations involved in every-day concepts. Such a discipline will involve in the first place the selection of those operations which are to be recognized as fundamental—probably the selection of the best operations for this purpose will involve a good deal of trial, and there will be delicate decisions to make. For instance, in giving meaning to the future I think the operations of making plans or drawing up programs or forming expectations for the future must be recognized. It will be easy to fritter away a good deal of analytical energy in

discussing to what extent this operation of planning is verbal, a question which it seems to me is unimportant.

Not only may there be verbal terms useful as intermediates in our thinking, but in society as at present constituted a great many terms about which we are mistaken in feeling that the operations are "objective", whereas examination shows that they are verbal, are nevertheless of the greatest social use because by the use of them with their mistaken implications we can procure the action from our fellows which we desire. However, in spite of their usefulness, I think that the common tacit ideals with respect to language are such that most people would feel that such terms are illegitimate, and that they should be discarded, once their operational makeup had been recognized. Most usage of such terms may perhaps eventually disappear if the race ever achieves a higher intellectual plane, but that it will be a slow process one may see by imagining with what enthusiasm demagogues would welcome the abolition of such terms.

In summary, it appears that we are not dealing with anything new or definite enough to be dignified by being called any kind of an "ism". It would be difficult to exhaustively characterize the operational technique of analysis; it is more of the nature of an art to be learned by the practise of it and by observation of its practise by others. It certainly does not attempt to set up a theory of meaning or to be an epistemology. In so far as it is concerned with meanings it is concerned with necessary as distinguished from sufficient conditions. So far as it is anything definite at all, it is a technique of analysis which endeavors to attain the greatest possible awareness of everything involved in a situation by bringing out into the light of day all our activity or operations when confronted with the situation, whether the operations are manual in the laboratory or verbal or otherwise "mental". It is an art to make the analysis emphasize the significant operations. The conviction that such an analysis of operations is important is to a certain extent characteristic of the scientists of the present epoch—the old Greek philosophers would not have felt this way. The value of an operational analysis is often to be found in the fact that it allows us to profit more easily by our

general experience—our experience has shown us that certain sorts of operation are good for certain purposes. If the analysis shows that we have been mistaken in the operations which we had supposed were involved in certain concepts, as it often does, then our experience enables us to revise our estimate of the adequacy of the concept to accomplish what we wanted. Operational analysis is valueless without a background of experience, and the conclusions from such an analysis can have no validity which is not already conditioned by the experience. The situation here on a larger stage is much the same as on a smaller stage with regard to dimensional analysis, for this too enables us to capitalize certain sorts of experience, but without the experience it is impotent.

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Physicists and Fairies

BY

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John Jay Chapman to *William James:* "You said something about a *concept.* Now what is a concept? . . . *Are you sure that there is such a thing?* If . . . the story of one of these concepts were brought before you, would you not . . . quench and dissipate it, and show it to be a mere mist and vagary and never-twice-alike will o' the wisp? . . . I can just imagine your polite 'not proven.' . . . But when you get on your tripod, you go puffing out these things at the top of the smokestack in perfect unconcern." (*Harper's Monthly Magazine*, Dec., 1936, p. 52.)

William James to *John Jay Chapman:* "A certain witness at a poisoning case was asked how the corpse looked. 'Pleasant like and foaming at the mouth' was the reply. A good description of you describing philosophy. . . . There are concepts, anyhow." (*Letters of William James*, II, 321.)

I. FAIRIES

WHEN the layman reads a book or two of popularized physics and then moves solemnly forth, as occasionally happens, to expound some comprehensive doctrine purporting to be built directly out of the materials he has picked up, the type of comment which the physicist will make is plain enough in advance. But why does it so rarely occur to the physicist that others may think of his epistemologizing much what he is sure to think of their quantizing?

The odds for intelligent statement are all in favor of the outsider and against the physicist. Physics offers the world a large amount of firm knowledge. But the philosophical and psychological sectors of inquiry have nothing of that kind to offer. They are at best in a Copernican, perhaps rather in a Ptolemaic, stage;

their keenest investigators know this, and from time to time we find one or another of them breathing the hope that now at last his science is attaining some measure of Galilean directness and simplicity.[1]

The objection is not at all to either physicist or psychologist going into the other's territory, nor to his using as best he can what he finds there. The two fields of work are drawing closer, problems of increasing difficulty are arising, and aid from many sources is needed. The risk is great, but the forays into surrounding fields are apt rather to increase in the near future than to decline. What needs criticism is rather a gullibility in accepting without factual check materials that are tricky enough in the hands of their primary users; and a solemnity in taking a dogmatic stand upon issues which are so obscure, even to the philosophers, psychologists, and logicians who debate them, that these gentry themselves rarely profess to understand one another when their arguments run to a finish.

Psychology—at least the kind of psychology that the physicist is most apt to get his fingers on—deals mostly with fairies, sprites, and spooks. Some of our psychologists, rhetorically speaking, are born fairy-minded, others lapse into it, while still others have fairy-mindedness thrust upon them; it is almost a necessity for their academic and professional survival.[2] I have not the slightest objection to a dainty Titania or a merry Till Eulenspiegel when the time is ripe, but these stodgy spooks that parade the field of modern inquiry in solemn pomp are another matter altogether. They are reminiscent of the mercury of the philosophers which purported to be the soul or essence of ordinary mercury, of vitreous earths, and of volatile principles; they are somewhat less reputable than was, in its day, phlogiston, the principle of inflammability—not fire itself, but that principle of fire by which combustible bodies burned.

The particular spook I propose to examine in this paper is the

[1] Thus: J. R. Kantor, "Principles of Psychology", 1924, vol I, p. xvi; Kurt Lewin, "The Conflict between Aristotelian and Galilean Modes of Thought in Psychology", *J. Gen. Psychol.*, 1931, vol 5, 141–177.

[2] The psychologists often try to camouflage their fairies. The physicist usually takes his in primitive forms. The difference, however, is not great.

concept. This is the one that physicists take so confidingly and regularly, as if by common assent, into their households.[3] The "whence and what art thou?" they do not ask; the "execrable shape" they do not suspect. They never seek to get a specimen in hand. The traditional philosophical and psychological abracadabra suffices to compel their belief. A word or two will be said later about the metaphysical pit from which the concept was first hoisted, but in the meantime the interchange between Chapman and James placed at the head of this essay will suffice for a warning that all the sailing is not so plain.

II. FACTS

Physics deals with facts. Fact, conversely, is what physics establishes. Thus our physical knowledge stands securely as our soundest knowledge. These statements, separately or together, in one manner of emphasis or another, have long stood firm for the range of our inquiries into "nature."

Modern relativity and the quantum have brought difficulties; but the very difficulties are invigorating; they enable us to inspect more closely than ever before the issues of 'factuality' itself. These issues display themselves in three historical stages, all of which, of course, have correlates at hand contemporaneously for our examination. The first is that in which ordinary perceptions under ordinary descriptions, such as rising suns, flowing waters, and smashing stones, are taken as basically factual; the second is that in which, upon a framework of superfactual space and time, the Newtonian lengths, durations, and masses, and the velocities, accelerations, and forces, take rank as the most assured 'facts' of the universe; the third, finally, is the very recent

[3] Those who have only casual acquaintance with the word 'concept' will be surprised both at the extent and at the chaos of its use. Among 22 contributors to two issues of this magazine in 1936, 18 used the word from one to 40 times each. Of the others, one used a still cruder word as substitute, two had very short contributions, and only one was notable for abstention. Classifications of its application or 'meaning,' based on a wider study of contributions, gave a) slipshod, b) impressionistic, c) dishonestly evasive, d) honestly hesitant, and e), in the very rare case, cautiously constructive with respect to 'postulate' and 'theory.' Logicians were the worst offenders, exhibiting at times incoherences of so criminal a type that, in comparison, the exhibits to be offered later in this paper drop to the level of petty misdemeanors.

stage in which the great superfact itself is brought back to its original dependence upon observer or observing mechanism, and in which, more annoyingly still, determinate positions and momentums, when jointly sought, demand capacities from the observer greater than he is able, under sound rules of workmanship, fully to provide.

I spare the reader expansion of these statements or even any attempt at precision for them, and content myself with remarking, (a) that in the first stage the 'fact' was always 'experienced fact' so far as the individual observer was concerned, however rigorously 'externalized' it was in its description, and that it always involved the tacit assumption that the individual experiencer was a good center for the universe and a good guarantor of actualities, however trivial he might be otherwise; (b) that, in the second stage, the grandiose absolutism was merely a provisional isolation of certain phases of the experienced fact in order to get better working control of other phases; and (c) that, in the third stage, not only do the reputed absolutes again find their way back into ordinary life from whence they came, but the earlier rigidities of externalization are themselves dissolved, and this with no harm to the virility of the 'facts'—*teste* the steady progress the photon makes into domestic service.

Much excited discussion is natural in such a situation as this, with epistemological optimisms and epistemological pessimisms entering a new phase of their ancient war. Such discussions arise, however, in the narrow background of the conventions of the generation, and I confess to little worry over them or their outcomes. I have read a book, and I know that the excited armies of words march up the hill and then march down again, and that the pillars of the temple do not fall. The fact-finders in person look tolerantly on, and their work proceeds unhampered when the time is ripe.

What concerns us here is solely the manner in which physicists reach out for psychological aid—and the type of aid or hindrance that they get—in this new situation which introduces a human variable, or a variable that seems and may possibly prove to be human, into their equations. We shall seek a display of what is

taking place in physical interpretation, with close confinement to actual pages written by actual physicists. The inadequacy and weakness of the daemons which the physicist summons to his aid can in this way most clearly appear. This procedure seems unkind to the particular physicists chosen for exhibit. Perhaps they will feel no need of solace, but if they do, they can find it in the fact that they are chosen not on their own account but because they are typical of many others, and because they hold the most prominent place in the particular locus in which I have sought my immediate materials, namely, the pages of *Philosophy of Science*. Understand, then, that my sole purpose here is to show what the fairies do to the physicist when they get at him.

III. BRIDGMAN

So far as my observation goes, there is just one recent physicist who, in extended endeavors to interpret the status of the new knowledge, has in any sense dealt 'factually' with the phenomena which he employs from the region of—or better said, from the vocabulary of—the 'mental.' He is the one who has shown the least yearning for 'mental' support, and the least dependence on it, which is probably a sufficient reason for the degree of 'factual' success he has had. Even in the case of this man who is, of course, Percy W. Bridgman—the early Bridgman of the "Dimensional Analysis" (1922) and of "The Logic of Modern Physics" (1927)—he has been so steadily worked on by the Brethren of the Hobgoblin that in his latest book, "The Nature of Physical Theory" (1936), one finds it difficult at times to keep the bold scheme of his criticism and construction free from the fuzzy clouds surrounding. His early work identified and described the 'haze' in ordinary observation more efficiently than anyone else had ever done. There he was dealing with the 'human' as he found it; but the added degrees of needless obscurity in the later work are not the 'human' that he finds, but an intrusion of 'bad human talk' that should be got rid of—indeed an intrusion of the very type of bad talking that he had so successfully attacked in earlier work through his displays of meaningless questions and concepts.

What Bridgman did in his early work was to say in effect: "Space and time are right here in my laboratory just as much as they are off at the far ends of the universe. Einstein has destroyed the old self-subsistent space and time I grew up to know. If such a thing as this can happen to the space and time that is right under my hands, then what tests do I have for the validity of my other physical knowledge, and what assurance for its permanency?" Bridgman thus generalized the issues of relativity, and later of the quantum, with respect to himself as observer and operator, in a way that Einstein himself not only has not done, but has rigidly refused to consider as of physical importance.

Bridgman was a worker with high pressures. What he did, he did with his hands. What he talked about, he wanted to talk about competently. This much of bias, if that is what it should be called, we may allege against him from the start. His method in getting his answers was very simple. He just pushed away all the rubbish he found around him without any feeling of awe for it at all; he made fresh, direct observations of the way physicists actually work; and he asked the kind of questions the worker in the laboratory wants, and needs, to have answered.[4] Then he established procedures for answering with a minimum attention to spooks of any kind. In doing this he gave 'Empiricism' a modernized dress, both physically and psychologically, and set up programs which are deeply influencing psychologists as well as physicists, and which by all the signs will grow greatly in influence in the future, if their main lines can be kept clear of distortion.[5]

[4] See a review of Bridgman's latest book by Karl Darrow in the *Review of Scientific Instruments*, 1936, p. 374, for a physicist's opinion of Bridgman's critical procedures closely akin to that which I here assume.

[5] In the pages of *Philosophy of Science*, and without any effort at a complete survey, I have noted about a score of writers naming him, or taking positive orientation towards his operational procedure. The chaos of appraisal is astonishing. The opposition of Margenau and Lindsay will be examined later in the text, and Malisoff's extended criticism will be referred to. Among psychologists, Tolman (II, 364) lists Bridgman as a "logical positivist" and proposes to parallel in psychology the good job Bridgman and his associates have done for physics; Stevens, however (III, 93, 100), casts out the logical positivists, but retains Bridgman, and is inspired to exhibit a psychological operation, namely 'discrimination,' which will be at one and the same time the foundation of psy-

The wide attention Bridgman has received is in part due to the appeal of his terminology, and more to the fact that he was early in his special field of criticism. His matter-of-fact procedure had the effect of providing his very empiricism itself with an empirical origin, so to speak, which was in sharp contrast with the highly rationalized empiricisms of the preceding generations.[6] This characteristic of his work, particularly as it is exhibited in the psychological range, is the one which he himself styles "naïve"; yet it must be regarded as immeasurably superior to the 'sophistication' he manifestly feels he has attained in his later work. This early "naïve" approach we are now to examine, keeping always in mind that we are not now interested in his own beliefs or professions of faith—of which a further word later—but solely in his actual practice with hand and pen.

Psychologically what Bridgman did was to make use of the behaving physicist just as he found him in the special case of "himself in his laboratory"—a living, breathing, working organism, dated to his nation and generation, trained to his profession,

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chology and of 'operationism' as well. Among physicists, Bender (I, 259, 265–6) accepts the operational view, but sees a possible differentiated place for the 'configurational.' Northrop (III, 230), discussing probability in quantum mechanics with the aid of 43 'concepts' in 17 pages, and with no attempt whatever to indicate what a concept may be, assigns what little plausibility he is willing to admit for the 'operational theory' to the manner in which its proponents hide the fundamental distinctions. Reiser (I, 199, II, 118) holds that an operational theory of concepts merely limits the range of application of terms. Feigl (I, 20, 424) rates Bridgman as an enlightened mind; Benjamin (I, 232) thinks Bridgman's 'operational definition' is "simply an act of thought"; Norris (III, 286–7) generalizes all logic as 'operational.' Other references or comments are by Wheeler (III, 53), Rosenzweig (IV, 105), Frank (II, 210–11), Carnap (IV, 38), and Gruen (III, 331–3). An inspection of the first fifty papers in the magazine showed sympathy and antagonism about equally divided. The opportuneness of Bridgman's discussion is shown not only in the wide adoption of his specialized word 'operational,' but by the way various philosophers and logicians seek affiliation with him, by the way certain psychologists and biologists pattern after him, and above all by the vigor with which rationalists of all schools unite to belittle him.

[6] Space cannot be taken in this paper to discuss Bridgman's "Dimensional Analysis", but no appraisal of his main theory is well grounded which does not build directly from this work. The word 'concept' does not appear in it at all, and the word 'operation' only in the early chapters and in a special technical application. The objective throughout is, however, to expel the mysticism, and allot the physical dimensions a sound operational status.

and set at a definite position in a long, historical line of scientific advance. Bridgman opened his eyes and took the man he saw—himself—and put him to work 'performing operations' and 'having concepts.' He made little effort to adapt his views to what the well-tailored mind is supposed to be thinking in this generation; he made inquiries, it is true, but found no nutriment in what was offered him, and let it lie. In this early development he made no use at all of the ancient psychological man, possessor of a 'mental' faculty, nor of that pseudo-man's successor, a brain, or musculature, or neural or vascular system, or perhaps a "whole-organism' presented as a gnomic[7] 'plus' to the organism proper. Neither did he use the faculty-man's ancient rival, the idea-kaleidoscope, nor this fellow's recent successor, the mechanist's gearing of reflexes.

To develop his interpretation Bridgman adopted the two key-words with which everyone today is acquainted, 'concept' and 'operation.' What the world has to be thankful for and in my judgment will long appreciatively acknowledge is that instead of hunting up meticulous definitions for these words, and then doing his best to degrade himself to the level of his definitions, Bridgman shook the words out like two signal-flags, and planted them, one on each side, to mark out the territories that must be taken into account.[8]

The word 'operation' indicates the thing-happening, a specific case of the physical fact as it is taken up by Bridgman for interpretative inquiry. The word 'concept' indicates any specific case of the presence or registration of the fact-as-known. In a way, then, but not at all in any of the ordinary ways, 'operation' represents the empirical, and 'concept' the rational element of the older disputations. The great difference is that the two are not split apart, detached; they are stresses in the one common situation of man-experiencing-fact. And right here is the point at which the primary Bridgman 'feel' for his problem is sympathetic, physically and psychologically, to the great requirements of the undertaking.

[7] Take either 'gnome' you prefer as base for this adjective.

[8] In this he is at the far pole from the 'logical' positivists who claim affiliation with him.

'Operation' is certainly in the range of things-sensed, but it is with equal certainty in the range of things-planned; and it is planned-things in the specialized forms they have attained as the outcome of long human experience and history. It is experiment and it is fact, and it is technique for getting at fact; it is nature itself passing through the laboratory, but it is nature channelized in apparatus, with the laboratory itself as the sum-total of the apparatus at work in the channeling. Operation is thus experience with empirical emphasis, but it is not at all the ancient and absurd 'passive' receptivity assigned in theory to fictive 'senses,' nor does it consist of the equally ancient and absurd isolations of fragmentary experiences alleged to be 'facts.' What Bridgman put to work in the laboratory was not a robot; neither was it a medicine-man; it was the physicist at work with his hands—himself a phase of the on-going natural process. He put him there with the clear-eyed recognition that where hands are at work, there head is at work also—a simple factual observation that our 'mentalists' never can tolerate.

How about 'concept'? Concept also is something the physicist has or does; and just as operation involves planning, so concept involves empirical experiencing—fact—outer world—call it what you will. Simple evidence of this is the ease with which the words "the concept of" can be dropped out of many passages in Bridgman's texts with no noticeable alteration of meaning; as, for instance, where in place of "the concept of space" one may read simply "space." 'Operation' and 'concept,' therefore, exhibit the characteristics of a common psychological world and of a common empirical world, so that some real possibility exists of development in a common system. They represent, nevertheless, different phases or aspects, of the types most commonly spoken of as "fact" and "knowledge of fact." For the term 'concept,' its extension, the range of its meaning, is plain enough in Bridgman's use; the sum total of concepts is the sum total of knowledge existing at any time.[9]

I have given a great deal of attention off and on for many

[9] For example: "The Nature of Physical Theory", p. 9; "At any moment our concepts are coextensive with the system of existing knowledge."

years to Bridgman's words 'concept' and 'operation' with a view to their future possibilities when some one finds a way to produce a really intelligible psychology; and the best way I know to characterize them, in their signal-flag values indicated above, is that operation represents what goes on in the laboratory and concept represents what goes on in the library, the physicist himself in each case being present and at work.

In such a set-up as this it is plain enough that two great lines of inquiry open up. One is into the operations as they affect the concepts; the other into the concepts as they affect the operations. It is equally plain in Bridgman's work that he only undertakes the first of these two inquiries, that being in the field in which he himself controls expert accumulations of specialized knowledge. The other line of inquiry remains open, and is an enormously important line. But it is not sufficient to pick some kind of a concept off the ancient concept-tree to stick in. Much less is known about concepts today than about operations, and much difficult research will be needed before good constructions can be obtained. As to this, a word or two will be said later on. Instead of making a specialized inquiry in this direction, Bridgman just assumes that knowledge in the form of concepts is somewhere around in the neighborhood, and goes ahead. Adopting a common form of word-stress which he himself now and then uses, we may say that in his theory he employs a *concept* of an operation, but only a *notion* of a concept.

As to his own view of the 'real nature' of his concepts there seems little question. At almost any stage of his career, if one asked him, his answer would doubtless be to the effect that they were a sort of little things kite-tailing along somewhere around a brain. The point about his work—the outstanding point—the great achievement—is that in his original construction he did not use in any way or attempt actually to use these little spooklets. Issac Newton oozed theological speculations and worked long and hard at numerology, and indications remain that he may even have thought better of his output in these fields than of the Principia. But who cares today? The point is that the vermin were kept out of the mechanics and the cosmology. So with

Bridgman. He did not let his 'mentals' and his 'consciousness,' nor even his 'self-consciousness' and his 'solipsistic' soul, operate in his early work; and so long as such things do not operate they do no harm.[10]

In his inquiry Bridgman occupied himself with physical concepts without attempting to differentiate them sharply from non-physical concepts, if any; he did not even take the trouble to tell us definitely whether he was concerned with *all* physical concepts, or solely with the good ones, assuming some are bad; he left open various issues as to the presence of 'mental' operations alongside the physical operations. These uncertainties are all to his credit, in the sense that an open chasm is less dangerous than one that is camouflaged.

His conclusions themselves he put in a great variety of phrasings, all impressionistic. These are so well known that all I need do is to recall such assertions as that the concept "means" a set of operations, or is "synonymous with," or is "equivalent to," or is "defined in terms of," or "names a group of" operations; or again that operational meaning is "the analysis of what we do," or is found in "making application," or in the statement that we can "maintain correspondences." Here again the very impressionism of his presentation is to his credit. It might be summed up in the statement "you can best keep your head by using your hands." This may not seem such a wonderful delivery; but the merit of it will shine when we come in a moment to examine what happens in the case of a physicist who insists that his head occupies a separate compartment from his hands, and must be appealed to all by itself in cases of deepest need.

Bridgman and Pavlov are akin in a very important characteristic. Pavlov pushed physiology out into psychological territory, and it was precisely because he refused to do any psychology directly that he became one of the world's greatest contributors to psychology. In a very similar sense Bridgman pushed physics out into psychological territory, and just to the extent that he

[10] Bridgman is criticised both as "too subjective" and for "expelling the subjective." The one type applies to his incidental phrasings; the other expresses the temperament of his critic.

lets the psychologizing alone he gets sound psychological results. The 'Pavlov' I have in mind is, of course, not the caricature offered us by the petty mechanists of the American psychological laboratories[11] any more than the one offered by their distortionist rivals, the 'purpositivists.' The real Pavlov is the grand old peasant who saw what he wanted and who went after it, and who was never satisfied until he got things under his own hands in a way that enabled him to talk definitely about them.[12] He first worked with stomach secretions and gained the Nobel prize. He next worked with salivary secretions in dogs, still always the physiologist. He then made the blunt observation that if a dog salivates in response to the presence of meat on his tongue, and if that is a physiological process, the process ought to be just as truly physiological when the dog salivates to the sight of meat two feet away. Why insert a spook in the latter case and not in the former? To answer this, he just eliminated a couple of feet of rigid Newtonian space from his investigational background; and incidentally he did that long before Einstein gave the rest of us the tip.

Pavlov quite early made a further observation which was that when his colleagues in the laboratory attempted to report results in terms of what the dog 'felt' or 'wanted' or 'saw' or 'thought' or 'knew' they never got anywhere; their reports were always vague and imperfect so that no two workers could come to definite agreement they could stick to. So he issued a rule: Cut out all the bad language in this laboratory, and talk facts. His original collaborator quarreled and went off on his own account; his students were always violating the rule; he himself kept slipping; finally a penalty was imposed, kopeks and rubels, for every offense, and in time precision was obtained in a considerable degree. Outside this 'mental' range for the dog, there was much of his work for which Pavlov could not get definite terminology; here he

[11] For current carelessness in discussing Pavlov, see some comments by Liddell in his essay on the conditioned reflex in the volume "Comparative Psychology" edited by Moss.

[12] This does not mean that Pavlov was a scientific paragon. He was always sticking out his neck, getting into trouble, and jerking back. His laboratory made some serious mistakes, but he was never ashamed to admit it and start over.

made no pretenses, but did the best he could and shifted as need arose. Bridgman's best work is like that of Pavlov in both these linguistic attitudes, and Bridgman's later reversion to mentalist terms only serves, it seems to me, to point a moral to the tale.

IV. THE BIRTH OF THE CONCEPT

Before passing to our remaining exhibits we may spend a moment to consider how the concept got into the world. It was not always with us, as some faithful souls so strongly feel. The case was one of mountains in parturition, a mouse being born, but What-a-Mouse! The Middle Ages struggled with their pains for centuries before they recovered sufficiently to attend to the housework again. Men had words, and some of the words were nouns, and some of the nouns named 'things,' or at least they named what the men of those days were in the habit of calling 'things.'[13] What, then, did the other nouns name—those upstage nouns such as 'animal' or 'virtue'? The official view was that the upstage nouns also named things, and upstage things at that; the heretic view was that the upstage nouns were just words, and that this was all there was to it. Realism on the one side, Nominalism on the other. Along came Abelard, at least so the story goes (for he was the publicity man who got all the headlines) to produce and exploit the 'concept.' Here was one of the great verbal triumphs of history. This 'concept,' present in mind, was to be '*the* thing' that the upstage noun named. Thus thing and word were to be alike happy, and each was to have its due. This was plain shenanigan, but it got by.

The British line of descent was in a by-path around the practical-minded nominalist, Hobbes, through the spawn of 'ideas' that the conceptualist, Locke, let loose in a Newtonian framework, and past the epic campaign which John Stuart Mill waged

[13] Such 'things' we have still with us today in many places where they ought not to be. A pleasant confession of faith from the eighteenth century is that of Thomas Reid: "Every object of sense, of memory, or of consciousness, is an individual object. All the good things we enjoy or desire, and all the evils we feel or fear, must come from individuals; and I think we may venture to say, that every creature which God hath made, in the heavens above, or in the earth beneath, or in the waters under the earth is an individual." (Essays on the Intellectual Powers of Man; Essay V, chap. i).

against the shades of Sir William Hamilton, in the course of which he emitted his despairing lament over the wreckage of the human intellect.[14] When George Henry Lewes wrote his history of these events he refused to tolerate even so much as an inkling that anybody could ever have been free from chronic concept-on-the-brain.[15] Today the common sense of the common man never dreams that a word may possibly be something other than a sign of such a concept, or thought, or idea, or whatever he may call it; while in the American laboratories not a tired rat can fall from his perch but some bright-eyed young researcher will discover in him "a concept just like a man's," with never a thought as to what that may be.

The physicist who fails to get the meat of this brief narrative may perhaps be willing to recall the verb 'phlogisticate' from the earlier history of his science, and understand me to mean that 'conceptualize' and 'phlogisticate' are piggies from the same litter. If he thinks I am wrong, all he has to do is to show me a concept in just the same sense he might ask me to show him phlogiston, if I insisted on its presence; which means, of course, showing something definite, and not merely waving one's hand towards a general region of human behavior to which such a word as 'concept' can roughly apply. The physicist is closer to an electron than he is to a concept; he is closer to a light-wave; he is closer to a probability-wave. He has more good sound practical reason for believing that he can some day grasp a 'probability' in his strong right hand than that he will ever make contact with a concept in person. All he needs to do is to stop believing-out-loud, and try to observe instead—just for a moment.[16]

[14] Hamilton had held at one stage of his work that concepts arise from judgments, and at a later stage that judgments are comparisons of concepts. Said Mill: "Coming from a thinker of such ability, it almost makes one despair of one's own intellect and that of mankind, and feel as if the attainment of truth on any of the more complicated subjects of thought were impossible." (An Examination of Sir William Hamilton's Philosophy; Vol. II, Chap. xviii).

[15] "It seems almost inconceivable that an acute mind could believe . . . that names . . . are not . . . signs of ideas" (The History of Philosophy, Vol. II, p. 26).

[16] I acknowledge that several times in print I have indicated what it is I regard a concept to 'be.' I shall try to exhibit the situation in detail at some future time. The difficulties are increased by the fact that almost all programs of psychology in the last two generations have evaded the issues.

Such is the concept which Bridgman found and casually adopted, but from which he stripped most of the hokum. Without investigating its procedure directly, he allotted it some incidental work to do, but rejected all its claims to dominance.

V. MARGENAU

We turn now to two physicists who are deeply pained by such irreverence and who, in their blacker moments, see in it the overthrow of all knowledge and the destruction of all possibility of attaining knowledge.[17] Margenau says little of Bridgman,[18] but has his eye steadily on him, and brings up the heavy artillery. Lindsay sniffs a beast in the underbrush, dubs it "operationalism," calls it the offspring of Bridgman, and starts to hunt it down with sidearms. Both men place all their faith and hope in the 'concept' which they insist must be restored to its ancient

[17] Henry Margenau: four papers in *Philosophy of Science*, as follows: "Meaning and Scientific Status of Causality", vol. I, pp. 133–148; "Methodology of Modern Physics", vol. II, pp. 48–72, and pp. 164–187; "Critical Points in Modern Physical Theory", vol. IV, pp. 337–370. R. B. Lindsay: two papers in the same journal: "The Meaning of Simplicity in Physics" vol. IV, pp. 151–167; "A Critique of Operationalism in Physics", vol. IV, pp. 456–470. To show the source of citations from any of these papers, the page number in parentheses will be inserted directly in the text. Lindsay's fear is expressed thus: "A thorough-going adherence to the operational method . . . would necessarily imply the abandonment of the method of theoretical physics" (458). In their book "Foundations of Physics" (1936), p. 412, Lindsay and Margenau describe Bridgman's position as "a short-sighted curtailment of the realm of physics." Margenau has also three articles in *The Monist*, 1929, 1931, and 1932.

[18] In his *Monist* articles, Margenau describes Bridgman as "following Einstein and others," and objects to "the penumbra of uncertainty" and the "meaningless." He criticises Bridgman's stress on differential equations in the analysis of causality as "mere description" and as "renouncing all attempts to explain it in terms which it does not itself offer without the intervention of reason" (1929, p. 328). He proposes to improve Bridgman's "principle of essential connectivity" by renaming it "the principle of consistency of nature," and gives three reasons why the operational theory "cannot be tolerated" (1931, p. 16): namely a) that it would dissolve the world into an unmanageable variety of discrete concepts without logical coherence; b) that it would impede science by emphasizing the changeable; and c) that it would make physical concepts falsely appear to be different from other kinds of concepts. In his later articles in *Philosophy of Science* I have noted only one reference to Bridgman by name (62), but others will be found or suspected, as pages 139, 144, 170, 174, 343, 345. We may possibly include the remark (63) that 'constitutive' definition enables us to reason, seeing that it is the 'epistemic' definition alone that is alloted by Margenau to Bridgman.

throne, with 'operation' reduced to the status of a humble, even if very necessary, slave. The word 'concept' is used sparingly by Margenau (about once in two pages), but he has several alternate words, and his heavy specialization,[19] the 'construct,' dominates his entire treatment. As for Lindsay he produces 97 concepts in 30 pages of text, among them such specimens as "sophisticated abstract concepts," "intuitive and *a priori* concepts," "logically indefinable concepts," "instinctive and not analyzable concepts," " 'economy of thought' concepts," "symbolic concepts," and "anthropomorphic concepts," apparently finding in them the proper ammunition for non-operational, though still soundly scientific, knowledge.

By distorting Bridgman grossly enough, either man can, of course, readily destroy what he has distorted. Both men distort alike; first by insisting 'operations' must be all hands and no mind; second by alleging that no operation in this world can possibly have anything to do with any other operation, not even with its own repetitions of itself.[20] Bridgman may be destroyed either in person or in distortion, so far as we are concerned at the moment; what we are to observe is what the distorters do to themselves.

We shall begin with an examination of Margenau and we shall confine ourselves to the manner in which he undertakes his anti-operational interpretation of physical knowledge, to the coherence of his mechanisms, and to the uses he finds for his construction after he has obtained it. We shall be compelled to omit a

[19] I doubt if I could find a definite statement to this effect anywhere in Margenau's papers. Lindsay, however, explains the situation thus: "In the interesting paper of Margenau, the term 'construct' is introduced in place of 'concept,' presumably to emphasize the active constructive rôle of the mind in building physical theories. This is a worthwhile procedure. However the present paper will continue to use the more familiar term with the feeling that no essential ambiguity will result" (161).

[20] "On the basis of purely operational definitions, all concepts are strictly empirical and isolated," (Foundations of Physics, p. 412). Definition by operation "would dissolve the world into an unmanageable variety of discrete concepts without logical coherence" (*Monist*, 1931, p. 16). "Logically the operational method . . . implies that each concept is tied to a definite operation"; "the concept must be arbitrarily associated with a particular operation" (Lindsay, 458). Allegations such as the preceding merely apply the older association psychology or the newer psychology of mechanistic reflexes to the opponent without his permission.

showing of his physical objectives proper, and of his frequently interesting and valuable organization of the physical material; though to give a fair picture of his position all this should be included, especially by way of indicating how readily his normal physics[21] may be detached from the 'mentalist' and 'conceptualist' interpretations thrust at it, and how these latter exhibit themselves more as matters of 'temperament' than of either 'philosophy' or 'science.'

The first of Margenau's four papers analyzes causality with special reference to the quantum by the use of a construct, the 'state,' the development and application of which is his main contribution to the direct interpretation of the newer theories. The next two papers build a system of what he calls methodology, but which might better be called conceptual mythology, to underlie the construct 'state.' His last paper is oriented to issues of truth or fiction in physics; for these return to bother him in ways they never could have done, had the mythology been omitted.

What we should like to be able to do in a case like this is to examine the writer's *utterances*, which are not only the evidence but themselves the *very fact* of his construction so far as we can come into contact with it, with as much precision and care as a

[21] The basic differences between Margenau and Bridgman in actual physical criticism, so far as I am competent to judge, are very slight. Margenau's sketch of his methodological problem (48–52) is alone sufficient to indicate this. To make sure, however, we must first: a) eliminate all conceptualizings and other psychologizings on the part of either man as totally irrelevant; b) eliminate minor elements of technical difference insignificant for the main interpretation; c) capture and banish to some remote, desert island Margenau's overpowering, though partially veiled, interest in issues of 'reality' which, when all is said and done, are nothing more than naïvely formulated distinctions of "common language" (346) "between physical objects and the observer's knowledge about physical objects"; and at the same time we must eliminate Bridgman's recent quibblings of comparable nature. We should then have our problem stripped down to what is truly significant: viz., the issues of causality, differential equations, simplicity, consistency, connectivity, and correspondence in the strictly physical sphere. Along with these we could inspect Margenau's "observations," his "operations" in the two forms, "preparations" and "measurements," (356–8) and his "states" as he actually uses them, in close comparison with Bridgman's constructions. My present essay began with this type of inquiry; but its continuation is wholly useless until the mythology, mysticism, and sentimentality is cleared away; and when that is done, almost any physicist can be depended on to reach better answers in immediate examination than I can with protracted discussion.

physicist would give to a study of tracks in a Wilson cloud chamber. Let the philosophers guess at what men mean and then squabble over their guesses; but we, in sharp contrast, ought to get down to the hard facts of the case, namely the specific assertions that are expressly offered us. We want to find out in which way, or in how many ways, Margenau's sentences curve, and how steadily they hold their curvatures. To accomplish this within any reasonable space we must select and sort, perhaps at times arbitrarily; and we undertake this task here with the disquieting sense that no matter what attitudes we may in this way most firmly assign to him, he will still be able to dig up out of his texts other and contradicting sentences to allege that we are wrong.

A. *A Breath of Preliminary Unity.* Deep down at the bottom of the pit of knowledge Margenau sees a frightful chasm opening to still lower deeps. Is Nature "merely a complex of awareness" or has it "the status of transcendental objectivity"? (60).* To a similar question the early Bridgman had replied that the answer did not make much difference, and that the question was operationally meaningless anyway. Margenau adopts what he says is "the most cautious and possibly the wisest course" by decreeing Nature to be "*merely* the aggregate of our perceptible experiences." What light this throws into the pit the reader may decide for himself. Margenau makes the further suggestion that by "coining a new and strange term," *habita*, and substituting it in physics for *data*, much good would follow.[22]

B. *Essential Dichotomy.* Physical science, according to Margenau, begins with "perceptible matters of fact"; it then places these in "mental custody" where, "in the privacy of one's speculations," they undergo an "act of speculative creation" and become "endowed with abstract properties"; and it emerges again later in "the realm of perceptible facts" (57, 59, *et passim*). Such phrasings will answer anybody's purposes, anywhere, any-

* See footnote 17 for method employed in citing Margenau and Lindsay.

[22] In his *Monist* paper, 1929, Margenau mentions his acquisition of this term from a "philosopher." Unfortunately he only takes over a small part of the significance of the word, and its extended investigation would be fatal to his "mental custody." In this same *Monist* paper he is quite frank in asserting his start from data that are "parts of our consciousness," in which we are "merely having cognitions."

time, especially under the qualifications Margenau slips into his sentences from the very start; all depends on how they are developed. Margenau's development is to declare a "dichotomy" (50–51); an "essential distinction between two kinds of entity" (170); a "formal bifurcation" necessary "in principle" (179); not "philosophical" and not "artificial" but instead merely such that one cannot "get along with less than two classes of things" (187), and that one errs if one ever ventures a tiny bit to doubt the reality of the "ultimate" abstract (187).

One "kind of entity" lies in the general region of 'mind' where it includes such items as the "intrinsically non-empirical" (53), the "totally abstract" (169), the "imperceptible concepts" (55), and the "transcendental elements" or "transcendental notions" (52).

The other "kind of entity" lies in the region of 'fact,' or 'sense,' or 'perceptibility'; and while it is asserted just as bravely, it is not pictured so well. Margenau defies the world to get along without the "transcendental notions," but he allows their consort, the "transcendental objectivity," as we have just seen, to become somewhat blurred; and indeed in another passage (345) he even tells us that the split between physical systems and our knowledge of them is not nearly so deep as the general split of epistemology —which to my mind is a very considerable concession to make to the enemy. We find, nevertheless, on this fork of the bifurcation, the "totality of all matters capable of sensory perception" (57, 59), and these must have "attributes of extension and duration" (60) and are sharply contrasted with items that involve even "simple processes of abstraction" (166).

C. *Organization by Concept.* Whatever fundamental criticism Margenau directs against Bridgman depends on this "dichotomy" which is unflinchingly vociferous despite the slight lameness in one of its legs. But Margenau's difficulty is that, with too much dichotomy in its materials, science can find no way to make any advance at all. Therefore, having forced a divorce, he now has to engineer a remarriage, or at least some kind of a conventionally tolerated, if not fully solemnized marital status. (Mixed arguments make me sprout mixed metaphors, and I have no inclina-

tion to apologize, either here or for other passages in this paper.)

He accomplishes this by a process of 'conceptualization.' Under a strong declaration of freedom of choice, he establishes the "physical universe" in high estate as number-one Concept. This concept embraces two 'classes' or 'domains' of concepts—one class "Nature," the other "Constructs of Explanation"; and these he labels "data" and "constructs" for short (59, 61, 166, 174, *et passim*).

These words, "data" and "construct," have most important functions in his system. When he so desires they indicate 'concepts' as here. When he does not so desire, they are bifurcations (as in B, above). They are therefore notably disingenuous; and this is true of them, I believe, not only in his work, but in all those regions of philosophizing from which he acquires them.

D. *Chaos out of Cosmos.* For all this procedure one can find analogues in Hindu mythology, except that Margenau's course is rather from cosmos to chaos than from chaos to cosmos. The gods wanted to stir up the sea of milk to get hold of the philtre of immortality. Vishnu, masquerading as the big sea-tortoise, sank to the bottom to give leverage for the stirring-stick; Margenau's "aggregate of experience" will do well enough for Vishnu. Out of the sea of milk finally popped up a White Elephant; Margenau's elephant unfortunately rises dichotomized, bifurcated—even, I fear, a bit schizophrenetic. On his back, Indra, Margenau's concept-maker, takes his seat, and proceeds to entrust himself with dominion over the firmament and the atmosphere, and to hold the earth in the hollow of his hand—quite some forerunner of the modern scientist. Our next step is to see what sort of immortal scientificity Margenau-Indra achieves out of it all. Again his sentences need much sorting.

a) *The Data.* Comparatively little attention is given to these; sometimes they are taken raw, sometimes conceptually; it is hard to disentangle. They include items like "pointer readings," "experimental decisions," and "countings of individual events" (61). In one illustration matter is a datum and mass a construct (61; and compare 69, footnote). Sometimes the "smallest part of nature" turns up "not a datum, but a collection of data," which

seems hard on the bifurcator (66; though before the paragraph ends, a re-entry of the "single perceptible counter-part" is implied). Sometimes no experiment is practicable except by way of a theory (179) in which case datum is reduced to observation of the type which involves strict passivity in the observer, and this seems hard on the concept-maker.

b) *The Constructs.* The manipulation of constructs is Margenau's main activity. He offers three classifications of constructs proper, and a fourth classification, not of constructs but of "definitions of constructs," whatever such a distinction may mean. These are:

1. A classification of constructs into five manners of 'existence' (164–6). This is not put to direct use, though it has sound value in eliminating various possible confusions and misunderstandings.

2. A classification, described as "more natural" (166) and again as "more qualitative" (174), into "sensible," "pseudo-sensible," and "abstract." Archetype for this is that magnifico among classifications (though hardly a prize-winner in recent physics) into the 'absolute,' the 'relatively absolute,' and the 'relative.' What happens for Margenau is what always happens with classifications of this form. It will suffice to offer a sample or two from among scores:

> "All constructs . . . possess necessarily some of the characteristics of nature" (171).
>
> "The systems themselves" (abstract systems being here in view, and all systems being constructs) "no longer partake of the attributes of data" (175).

The Hamiltonian operator illustrates constructs that are "totally abstract" and "wholly insensible" (169–70); but it is "not entirely free from an admixture of sensible and pseudo-sensible constructs" (171); it involves space and time coordinates, and this is as it should be since "every abstract notion, if it is to be intelligible, implies some reference to sensory experience" (171). At a point like this nine days retirement to the desert for silent thought is indicated. The result of meditation will be that Margenau is obsessed by visualizability, that he merely suggests

three degrees of it, and that the classificatory pretentiousness adds nothing to the import of his suggestion.

3. A classification of "definitions of constructs," ancillary to a third classification of constructs proper, into two typical forms of definition called "epistemic" and "constitutive" (61–62).[23] The epistemic is approximately 'operational' and permits measurement, but the constitutive is much nobler since by it "we are enabled to reason" (63). The far echo of one of Kant's most famous phrases is in this wording. If the question is asked why Margenau offers a classification of definitions separate from that of constructs proper, the answer is probably that he has now reached the particular point at which he must command a dichotomy for his warfare against 'operational' physics; and since he cannot exhibit such a dichotomy in the 'constructs' directly, he pushes it back into the region of "definitions of constructs," (even though he avoids so much as the implication of dichotomy for the definition-types) and on that basis proceeds to manipulate the definitions speciously with respect to the constructs to uphold his cause. The definitions thus act merely as a 'front' in the old Wild West sense. This is at once plain from the uses to which they are put. He now obtains:

4. A classification employing the words 'system,' 'quantity,' and 'state,' which takes two forms:

4′; into systems and quantities, wherein the former permit constitutive definition only, while the latter may take both constitu-

[23] The word 'constitutive' is derived from Kant who uses it in an entirely different sense. The word 'epistemic' is not found in Murray, nor in the technical dictionaries of Baldwin and Warren. The coupling of the two words was made by W. E. Johnson in his "Logic" (1921 to 1924, 3 vol.). It is interesting to note that whereas Johnson introduced the word 'epistemic' to replace 'subjective' which he disliked, and made it indicate "the relation . . . to the thinker" (vol. i, p. xxxiv), Margenau attaches his own heavily subjective stress to the opposed word 'constitutive.' I mention this merely as a curiosity, not in reproach, since all such terms are so slippery it is impossible to expect anyone to hold steadily to them, not even when criticising them. Observe that Margenau first tells us that the two kinds of definition are "different," then that they are "interdependent," and then that epistemic alone will not suffice. His illustration for the electron is worthy of schoolmen in their prime, namely that 'it' cannot be measured, but that "we do not question the possibility of measuring its charge or its mass, etc." (63). Observe also his very first illustration in the case of 'mass,' where the 'constitutive' is granted superiority, even though requiring auxiliary 'epistemics' on which to rest (62).

tive and epistemic definition; and wherein the state appears as "a combination of quantities with a system" (64).

4″; into systems, quantities, *and* states, with the last of these "a third class of constructs" and with the comment that "it is a matter of practical indifference" whether we look upon the states in this way or "as associations of systems and quantities" (70).

Since 'state' is Margenau's greatest engine of war, his trusty catapult, one regrets that he should be so unclear about its status under his basic definitions. If (as in 4′) a 'state' is regarded as a combination or association in which one of its elements is already a combination, we certainly have a right to be told something about ratios, and whether the second stage of combination is merely additive, or involves, so to speak, a second power of the 'constitutive.'[24] If on the other hand (as under 4″) the 'state' is a third 'class,' then we should know its standing under 'definition,' if these definitions are to have the authority attributed to them. The sole schematic position Margenau has left open among his definition-types is 'epistemic only'; and the use of this would involve a surrender to Bridgman which manifestly is impossible. If a third type of definition is to be called on for the benefit of 'state,' no hint is given us of what this may be.

Difficulties like the above are academic and trivial, however,

[24] Margenau's "simplest possible way" to form a state is by "a direct assignment" of certain quantities to a system (64), but he assures us that no one-to-one correspondence between constructs and data is involved in his procedure (66). Later, however, he attains the somewhat surprising presentation of a "one-to-one correspondence . . . between states and probability distributions of observed properties" (341). Historically his definitions of 'state' run back to "an agreement to consider those quantities as composing the state of a system which enter into a time free differential equation describing its behavior" (145). Later he defined states for quantum mechanics "by means of *state functions*, functions of space and time coordinates which make no explicit reference to quantities" (175). And finally state or representative of state comes to mean in quantum mechanics "a certain function of a suitable number of variables" (340). Elaboration will be found in "Foundations of Physics" (401–413) where a "physical system" is "any object . . . which in classical physics is thought of as an entity," its "properties" being called "observables," and its "state" being "simply a function in configuration space." In this connection he also tells us that, if classical mechanics is like a report on a man's actions and utterances, the "state" in quantum mechanics gives us "a story of the succession of all his thoughts, sentiments, and volitions" (401–2). Maximum illumination is here apparently cast upon the situation.

compared with what happens when 'system,' 'quantity,' and 'state' are put to work. 'System' is handicapped by an unfortunate Siamese-twin-like start in life, since it is not only "constitutive only" but also an "entity," the "notion" of which is "largely carried over from previous physical experience" (Foundations of Physics, p. 401). In neither of its personalities, however, can it very well be quantity or do measuring, since, also from the start, the 'epistemic' is involved in that (63–4). And yet:

"All properties . . . with which physical systems are endowed must be measurable" (341).

"There is no consideration which compels that states be directly measurable" (341).

Of course Margenau has an "emergency exit," here as elsewhere. The first sentence above is in terms of "properties," not of "systems" direct, under the super-elegant distinction already noted in the case of electron-as-system.[23] This 'exit,' however, opens upon a road which runs from a starting point in 'reason,' where system is 'constitutive only,' past a rough spot in which 'properties' (341, *et passim*) crop out, to a terminus in which the systems have 'behaviors' of their own (339). On such a road we need skis, waders, and dancing pumps, all at once. But for the "dichotomies," travel would have been smooth. No doubt Margenau can demonstrate my account here to be false. In doing it, however, he will not 'refute' me, but rather illuminate still more brightly my exhibit of a linguistic mess, and of its uselessness.

We could perhaps overlook differences that seem to us incoherent at different stages of his development, or adjust ourselves to them without complaint since we are all errantly human, if only he would maintain separate coherences for the separate stages. But compare now these two sentences:

"A state function . . . is not at all linked with physical experience" (340).

"States which . . . fail to provide a *definite connection* with experience must be considered futile" (341).

Again there is a fire exit, but again one might as well stay behind and burn.

Margenau's idolized 'mentality' is both origin and shield for these conflicts, giving him hope, or perhaps illusion, that behind the murky words the mind's still shining. It is this 'mentality' which produces his concern over 'reality' in his final paper, a concern which is intense and unchecked despite the devices he uses to disguise his very preoccupation from himself. After constitutive 'system' has bobbed up as behaving 'fact,' we find him spending many long pages debating the relative merits of 'system' and 'knowledge of system' (344, ff.); while even for 'state' the comparable question comes up in the form: is it 'state' or 'representative of state' about which we are now talking? (340).

E. *And What Good Came of it at Last*? We have exhibited a chaos of inconsistencies and the presence of much wasteful rumination. It is further plain that Margenau violates every canon of simplicity. But he may retort that even though he exhibits inconsistency, wastefulness, and frightful complexity, this is the best one can do these days; and he may insist that despite all its defects the usefulness of his interpretative system still remains. The answer to this is the exhibit of its redundancy. After he has constructed his program, about all he does is occasionally to refer to it. Run through his illustrative matter (174–187) and see if this is not so. After an examination of "elementary diffuseness" which his section heading (174) suggests to be "a result of abstract physical constructs," he sums up: "By placing into prominence certain abstract features which have always been inherent in physical explanation it is possible to reconcile .... etc." (177). This is a sound statement, matter-of-fact and true; it tells exactly what he is doing, and incidentally what everyone else does, but it certainly does not exhibit physical facts in any way as the 'result' of constructs; it is unexceptional so long as he does not attempt to pursue the task, utterly hopeless under his 'conceptual' methods, of making a basic development for the word, or procedure, 'abstract.' Continuing further with simple, direct statement, he feels apology necessary for his use of a "customary,

technical lingo which obscures distinctions between constructs and data" (178), apparently not noticing that if this 'lingo' is proper to the extent he has used it, then the whole distinction between data and construct might as well fall out. He next (179) reasserts his scheme "in principle," but with the admission that "in actual investigations it is often" (he should have said 'always') "impossible to maintain a sharp boundary"; and he follows this with a practical statement as to how physicists make theories, but one which has nothing whatever to do with his own interpretative mechanism. Finally, entering upon a longer discussion of certain constructions of Dirac and Fermi, he frankly writes: "We shall exercise little care in distinguishing constructs from data . . . for the result would be . . . tedious . . . ." (181). His actual appraisals of Dirac and Fermi are noticeably 'operational' in tone.

Margenau's formal opinion has been that the only salvation of physics lies in a "dichotomy," but now after forty pages of dichotomic crutch-building he throws away his crutches and walks like anybody else—practically, instead of dichotomically. We may conclude that his theoretical interpretation has as little to do with physical science as a cutaway frock at a wedding has to do with the simpler facts of life. The world will always, one may suppose, include people who prefer the regalia to the reality. Margenau's essays will have served a useful purpose if they have demonstrated what regalia the physicist can most easily get along without.[25]

[25] Alterations in Margenau's views may be in part responsible for his conflicts in expression. Thus as contrasted with his early demand that physics abolish the penumbra of uncertainty (see footnote 18) he now occasionally remarks that his method has no finality (51, 68, 179, 339, *et passim*). Another difficulty is that at times he regards his program as philosophical (337, and the preface to Foundations of Physics), at other times as not philosophical (187), and at still other times as a method for keeping other physicists from talking inconsistently (338, 345, 346). In the analysis in the text I have limited myself to inconsistencies of a special type. For slightly different illustrations, note how certain formulations such as that of causality can be at the same time both tautological and statements about nature (146); and how Heisenberg's principle can be viewed as a "consequence" of a certain vaguely worded axiom Margenau himself formulated a decade later (341).

VI. LINDSAY

Lindsay makes direct assault upon a certain 'operationalism' he attributes to Bridgman (456, note). This may be treated much more briefly. It will interest us not only for the display of the weakness of the argument, but by showing how unscrupulous the anti-operational 'conceptualist' can become when he goes on the loose with 'ideas.'

An 'ism,' in the general case, is just a lumping, and so to speak, personalizing, of misunderstandings, as opposed to efforts at their clarification. One thing that Bridgman certainly is *not*, is the creator of an 'ism.' If he has ever used the word 'operationalism' a single time, I have failed to note it; and certainly it is alien to his entire attitude, since salvation from the 'ismic' disease would be one of the surest by-products of any steady operational stress on meanings.

Notice Lindsay's procedure in his first two paragraphs under his heading "Operationalism" (456–7). Watch the words wriggle. Beginning with broad generalizations in terms of meanings and definings, he is ready by the middle of the paragraph to assert that the content of an 'operationalist's' thinking is "glass and rubber tubing" and other "actual apparatus." He then admits that this is an extreme statement, and grants the operationalist other methods of defining than this. He next changes the issue to one of consistency, but asserts emphatically that the operationalist has now bound himself to believe very definitely that "no contradictions can ever arise in actual physical situations," and must be willing to trust himself "always . . . to the test of such situations." "This thesis" (meaning thereby apparently the chaotic contents of the two paragraphs) is the 'ism' which Lindsay proposes to examine and refute; and he proceeds in his attack without any effort whatever to explain to us what can be meant by a "contradiction" in an "actual physical situation" or why a physicist should find it so appalling to be asked to give his confidence to the "actual physical situation."

"Actually" Lindsay's arguments against Bridgman are as follows: He first assumes naïvely the existence of a certain "purely theoretical" element which involves "ideas in mind"

and "purely mental planning." Putting this "Charlie McCarthy" on his physical lap, and holding his breath till he thinks he has made us believe that his Charlie is radically different from "actual apparatus," he profoundly proclaims that no "apparatus" can get along without "ideas" (457). This proclamation we are supposed to accept as knock-out number one for 'operationalism.' He next asserts a rigorous isolation for all operations, each from every other, and thereby a second time puts 'operationalism' on the floor for the count (458). Fearing, however, that he may have gone too far, he proceeds to assure us, like a true Charlie, that theories require verification, that experimental tests are still useful, and that the goal of physics is still ultimately "to describe physical phenomena"; adding that what he really means to insist on is a reservation of his right to use "concepts which are not defined *directly* in terms of operations" (459). He is apparently unaware that this matter of 'directly' which I have taken the liberty to italicise in the citation, is the whole issue of inquiry. All he does is to assert a right of 'indirection' as over against a crude falsification of 'direct'; and there he stops, with no contribution at all to the solution. This, so far as I can find out, is his whole case against Bridgman's critical inquiry into physical knowledge.

He adds, however, a few minor considerations. He intimates that operationalists "flirt with the idea of emergence," leaving the implication that he is referring to Bridgman (465). Apparently he has not read Bridgman's own remarks on emergence ("Nature of Physical Theory," p. 97). He dislikes the way Bridgman talks about 'text,' because "although it is of course true," yet "it seems to give a misleading impression" (459). He criticises certain of Bridgman's attitudes towards models, probability and statistics, but in ways only slightly relevant to the issues between 'operationalism' and his primordial 'conceptualism.' He winds up with a summary of his overthrow of 'operationalism' by declaring a) that it is fascinating, b) that it casts light, c) that it has weaknesses, and d) that "certainly it would demand the complete scrapping of the well-recognized methodology of physics" (469–70). In other words he credits it with

three trivialities and an absurdity. It is little above the level of devil-worship to believe that any such 'ism' as Lindsay's imagination projects could ever have power to work such harm.

What this sort of 'conceptualism' does to its advocates is to distract their attention from honesty of statement, and in large measure destroy all their sense of humor. What it does to Lindsay himself may be made evident in three brief examples. In his article on simplicity in physics he arrives at the fine pedagogical conclusion that the theory in physics which is the simplest, and thus presumably the best, is that one which the average student can learn most quickly (166). In his later article he speaks of space and time in mechanics as primitive, indefinable concepts (459), which would seem to indicate very slight effect upon his thought as yet from the Einstein relativity. After having built his main arguments on the "purely mental," he proceeds to set forth all physical theory as "but an attempt to *describe*" (459), while in the very next sentence he describes description itself as beginning "with certain intuitive and logically indefinable concepts" and advancing to the building of "more elaborate concepts by purely postulational methods." If this be 'description,' then Robin Goodfellow is a molecule.

VII. BRIDGMAN AGAIN

We return now to Bridgman in order to inspect the later stages of his development especially as we find it in "The Nature of Physical Theory" (1936).[26] His direct criticism of physics remains as fine as ever; the reviewers give it all the good words: matter of fact, common sense, simple, direct, pertinent, hard-headed. Asserting the importance of the "physicist as critic" alongside the "physicist as theorist" he is in position to make sound progress on lines that recall Karl Pearson's use of Cousin's "La critique est la vie de la science" on the title page of the Grammar of Science.

But there is another side to his work. In place of the "naïve" viewpoint he formerly took, he now seeks to insert 'foundations' of a kind that require him "to map out the possibilities and limita-

[26] Page references in the text will be to this book.

tions of the human mind in dealing with the problems presented it" (2). The world's long delay in getting to close quarters with this type of inquiry is probable evidence how much more difficult it is than the problems physics itself has solved. But Bridgman is not timorous. 'Operationally' his indicated course would seem to be increasing caution and precision in the use of the words he draws from the surrounding psychological vocabulary in the general region he describes as that of 'concept'; but this does not appeal to him at all. He simply adopts 'mind' the way he has heard about it somewhere, and seeks results as mighty as Margenau's out of materials as frothy as Lindsay's.[27]

The moment he starts his examination he finds himself involved with 'thought' and 'language,' and with their status with respect to a something known as 'experience' (15). He is in the world's worst bog, but he goes merrily ahead. He gives us a chapter on thought and language, another on logic, and two on mathematics. Our complaint is not that he makes this inquiry, but that in it he employs all the bad devices he had ejected from physics, exploring, thus, "the properties of our mental processes" (13) by the aid of 'essentially,' 'vitally,' 'absolutely,' and 'intuitively.' The outcome for thought, language, and experience is as follows:

First Language must be differentiated from Experience. This is the way it is done (24):

"Activity is the basal property of all our experience."
"Language as language is divorced from activity."
"Language must be *language used*."
"*Language used* is obviously an activity."

The physicist, when he goes outside of physics, and whether he be concept-adorer or operation-priest, seems wholly without sense of linguistic shame.[28] The comment here must be the same as

[27] Reviewers of the book are almost unanimous in deprecating this development, even though each may have a different set of reasons for his objections. Malisoff's discussion in *Philosophy of Science*, Vol. III, pp. 360–364, covers the ground so fully that we may omit most of the details of criticism here.

[28] In fairness we must admit that psychologists and philosophers set him the example. In mathematics E. T. Bell is outstanding for inconsistencies arising from interpretative misuse of the 'mental.'

in the similar case for Margenau. If one must write contradictions such as these, why write at all? But this is not the worst. Bridgman next finds it necessary to differentiate Language from Thought, and this he does as follows:

"The primary tool of self-conscious rational thought" appears to be "the tying up of experience into bundles which are capable of recognition" (26).

"Language separates out from the living matrix little bundles and freezes them" (24).

If Bridgman's exhibit of "the living matrix" is different from his exhibit of "experience," or if his "tying up" is different from his "freezing," then I am criticising him unfairly. But I find no difference in his account. 'Operationally' he shows us Language and Thought as 'the same,' as far as he goes. If they are in any sense 'the same,' then he ought to treat them so. But he does not. Quite the contrary. He thrusts them very far apart, even though he has nothing but conventionalized emotion to support him. Consider this:

"The two are not identical; thought is infinitely the richer, for it may contain awareness of the continually shifting background of connotation that is incapable of expression in language. . . . Thought is thus closer to experience than language" (26).

These words are all muddy; and the implication that a thing "'is infinitely' this because it 'may contain' that" is certainly queer for a realist to give us. But observe also how easily the assertion can be turned upside down with equally plausible results, as thus: "Language is infinitely richer and closer to experience than thought because it joins issues with precision, sweeps the cobwebs away, and pockets its gains," these motley nouns and verbs being just as safe and sane as any Bridgman uses.

All depends on what one thinks is 'rich' or 'close.' Some like the gravy and some like the meat; but you can't build much muscle out of casual, personal preference.

Bridgman does, indeed, seek more definite statement than this, but never, so far as I have noted, without a hesitating quali-

fication. Consider the words I have permitted myself to italicise in the following:

"Modes of thought . . . are possible from which the verbal element is *almost* entirely absent"; also some which go on "without *consciously* getting onto the verbal level" (25).

"The use of language is an activity" but "the fundamental device of *practically* all thought" is "analyzing experience into static bits with static meanings" (58).

Minor illustrations of the outcome of such procedures as the above abound. One will suffice. Bridgman repeatedly examines certain questions about *pi* (41, 43, 47),[29] especially as to whether men had a right to say that it was either transcendental or not transcendental in 1881, the year before a proof was offered that it was. Somewhere in his discussion Bridgman applies his word 'meaningless,' but we can go a bit further than he does, and apply it even to his discussions of such issues, in which nothing can be found that is either appetizing or edifying. So vague is his wording that I would not even attempt to state precisely what the issue is. In front of me lie comments by four men, all no doubt impressionistic: one says Bridgman thought the question meaningless in 1881, another that it was meaningless to ask the question in that year, a third that Bridgman held the transcendental *pi* did not exist in that year, and a fourth that if he did not hold it non-existent, he should have, in order to be consistent. Bridgman is not that vague, of course; but his background of discussion is. He is far remote from operational procedure; and his recent shift in the type of his analysis bears most of the responsibility.

VIII. FAINT HINT

A physicist cranking an ancient car with worn gears and leaky valves would never say: "What a wonderful machine. Its 'spirit' is right. I will use no other." Why, then, when he needs a behavioral carriage for his science, should he hold to a broken-down psychology of antiquated model?

[29] See also his essay, "A Physicist's Second Reaction to Mengenlehre," *Scripta Mathematica*, vol. II, 1934.

What difference can it make to the physicist whether 'ideas,' 'concepts,' 'minds' really 'exist' or not, or whether anybody else thinks they exist or not? He has long since overcome his old imagined dependence on the 'reality' of physical objects as a necessary presupposition for scientific work; he did not overcome this dependence because he wanted to, but because he had to; because the only way he could acquire sound physical descriptions was by abandoning some of his older rigidities.

The sole issue of scientific importance with regard to the 'mental' is whether the mentalist vocabulary offers a sound technique for description or whether it does not. We know it does not. No one can put it to work properly. The more earnestly one tries, the better one understands its radical deficiencies. Modern logic offers a horrible example. We have examined what certain physicists have tried to do with it. We may feel assured that their failures are not those of personal incompetence, but instead of the tools they have attempted to use, and of their almost hypnotic belief in the efficacy of such tools.

The 'mental' cannot help other sciences because it cannot help its own. It is a breeding place of 'schools,' on the side of theory as well as on that of practice. Psychoanalysis is in illustration with its messes of Cs, Ucs, and Pcs, remote from possibility of scientific usage. Faculties and associations of ideas, reflexes and brain cells[30] have had sounder scientific purpose, but no greater constructive success.

The strength of the 'mental' in the past has rested in the certainty—as great a certainty as we can say we possess anywhere

[30] Brain and nerve research lies in one of the soundest regions of experimental psychology, and K. S. Lashley is outstanding in this field. Here is what he says of his own branch of inquiry: "It is doubtful if we know anything more about the mechanism of learning than did Descartes when he described the opening of the pores in the nerves by the passage of animal spirits. His statement sounds remarkably like the reduction of synaptic resistance by the passage of the nerve-impulse," "Handbook of Experimental Psychology," edited by Murchison, p. 493. Lashley's view in this respect is not isolated; rather it is typical of what outstanding investigators in any special branch of psychological inquiry will say of the status of knowledge in their particular branch. An examination of a dozen American psychological systems, under standards such as those that have been used in this essay, will be found in my book, "Behavior, Knowledge, Fact," 1935.

—that we confront actual phenomena, namely the behaviors of men, which do not attain adequate description in terms of spatial coordinates, and which thus, in a properly technical sense, do not exist "in space" at all.

The physicist may remind himself, however, that the 'space' out of which 'mental' phenomena have been ejected was the old absolute space-form of Newton, which was postulated as if something over and above all the events which happen within it. Despite the value this space-form retains, it can now no longer pretend to such great dominion. The new spaces that are succeeding it are much more tolerant to behavioral description. But they are more than this: they bring a new freedom to the enquirer into behavioral phenomena to construct and postulate whatever spaces he himself may need in which to examine his behavioral events; they come with the assurance that the sole criterion of their own validity and worth will be the success of the descriptions made by their aid.

Since the 'mental' as we have known it in the past was a squeeze-out from Newtonian space, the physicist may be asked to ponder how it can still remain a squeeze-out when the space out of which it was squeezed is no longer there to squeeze it out.

If this faint hint aids the physicist it will only be as he learns to distrust the reports of the pre-scientific psychology and do some of his spade-work himself.

Paoli, Indiana.

Causality in Field Physics in its Bearing upon Biological Causation[1]

BY

F. S. C. NORTHROP

THE concept of causality as it appears in a specific scientific theory involves two factors: (1) the relation of necessary connection between the states of a system at different times, and (2) the definition of state at a given time.

The first of these two factors is necessary if causality is to hold for a given theory, but the more interesting point philosophically is the second factor, since it tells us what initial conditions of a given system a given branch of science finds it necessary to assume in order to be able to deduce by means of the relation of necessary connection, a future state of that system.

Consider a few concrete examples. You will recall Laplace's famous statement to the effect that were an all-seeing Providence provided with a knowledge of the positions and momenta of all the particles in the universe at a given instant of time, He would be able with the help of the Newtonian equations to predict any future state of the universe to the minutest detail. In this dramatic expression the two factors are present in the form in which classical particle physics employed them. The definition

[1] A paper read in the symposium on *Causality in Physics* at the meeting of the Eastern Division of the American Philosophical Association at Princeton University on December 29, 1937.

of state at a given time is given in the knowledge of the positions and momenta of the particles of the system, and the relation of necessary connection is provided in the form of the Newtonian equations together with the relation of formal implication as applied to them in mathematical calculations.

In a paper on the concept of probability in quantum mechanics read before this association at its Baltimore meeting two years ago (1), the present writer, following Lindsay and Margenau (2), indicated the presence of the two factors. There we stated that if one means by causality not merely the relation of necessary connection between the states of a system at different times but also the particular definition of state which Newton gave, then most certainly causality does not hold in quantum theory. But if, on the other hand, one means merely the relation of necessary connection between states at different times, as would seem more appropriate, then quantum mechanics is to be regarded not as a rejection of causality but as an indication of the radical change in the definition of state at a given time which must be made in order to retain the concept. The relation of necessary connection between states at different times is given by the Schrödinger time-equation. The difference from Newtonian mechanics centers merely in the fact that in order to retain the relation of necessary connection as prescribed by this equation, it has been necessary to identify the state of a system at a given time with a function ϕ which is defined not merely in terms of positions and momenta or other measurable quantities but also in terms of certain numbers correlated with these quantities which designate the probability in any observation that a given observable value will be found.

It is to be noted that the retention of causality is not purely nominal due to an ambiguity in the definition of state at a given time. A state is none the less uniquely and unequivocally defined because two sets of numbers, rather than one, have to be given in order to determine it quantitatively and experimentally.

Recently a book upon quantum mechanics by the physicist Landé (3) has appeared in which this validity of causality is put in a slightly different but perhaps more intuitively comprehen-

sible way. Starting with the complementarity principle of Bohr, according to which any process may be expressed in the two independent parallel languages of wave or of particle and photon, Landé maintains that causality holds in both languages and only seems to break down when, due to the operational approach to the problem, one fallaciously muddles the two worlds of discourse, by bringing a process, such as the diffraction of electrons, up to a crystal screen in particle language and then completing the process from the screen to the plate in wave language. When this is done causality obviously fails since it is impossible to deduce a later state of a system defined in wave terms from an earlier state defined in particle terms, but carry the whole process through in either[2] language, Landé suggests, and the connection follows necessarily. This point is interesting not merely because it confirms by independent considerations our position following Lindsay and Margenau, taken two years ago, but also because it indicates the positive dangers to scientific thought in the opera-

[2] This is usually accepted for the wave language. Of the particle language Landé writes: "If the picture of particles is adhered to, then there *is* a mechanical causal explanation." (3) (p. 44). In evaluating this statement it is important to distinguish two senses of the word "particle." There is (1) particle in the sense of classical particle physics, i.e., an entity persisting continuously in space and time with a position and momentum at every instant, and (2) particle in the sense permitted by quantum mechanics. Of particle in the former sense one may say, speaking loosely, that causality does not hold in quantum mechanics for it, but the only precise way to state this point is to say that particle in the sense of classical particle physics does not exist for quantum mechanics and hence to ask whether causality holds for it in quantum mechanics is meaningless. There is, however, the other sense of the term "particle" which does have meaning in quantum theory, that, namely, of the entity to which certain quantities in the state function may be spoken of as referring when one chooses, quite unnecessarily, to express formal quantum mechanics in the language of discrete entities. Depending thus upon the state function for its own definition, and causality holding for the time relation between state functions, causality may be said to hold for the "particle language" as thus defined. Landé seems to be using the term in this, its proper, sense when on page 24 he writes: "It is only if one insists on clinging to the old mechanical models that one comes to such contradictory ideas as . . . 'failure of mechanical causation.'" On pages 44 and 45 and elsewhere it is not clear, however, that he avoids falling back into the classical meaning of the term. It would seem wise, therefore, either to drop the wave or particle language entirely, using only the formal exposition of quantum theory, or else to introduce two different words for the two different senses, perhaps reserving the term "particle" for the entity of classical particle physics and introducing some other term such as 'quanticle', for the particle of quantum mechanics.

tional theory of the *meaning* of the scientific concept. It is not an accident that those who have been most certain of the rejection of causality in quantum theory have been most uncritical in their acceptance of the positivistic theory of the *meaning* of all scientific concepts.

Although Landé's analysis is illuminating in indicating why certain people reached the conclusion they did with respect to the status of causality in quantum mechanics it is not as trustworthy a way to put the matter as the more purely formal one used in my Baltimore paper. The reason is that the Bohr complementarity principle is not required, since the formal statement of the theory in which all attempts at wave or particle pictures are dropped, thereby making the complementarity principle unnecessary, gives a more general analysis of the status of causality and takes care of the experimental data with a smaller number of formally precise initial assumptions. Also, the danger of falling back upon the rejected mechanical models by surreptitiously using the particle and wave of quantum mechanics in the sense of the particle and wave of Newtonian mechanics and hydrodynamics is avoided.

Up to this point we have emphasized the respect in which quantum mechanics retains the concept of causality by introducing a definition of state different from the one used in Newtonian mechanics. I now propose to direct attention to a respect in which quantum mechanics represents a return to the definition of state used by Newton. This brings us to the concept of causality in field physics, the major theme of this paper.

Ever since 1861 when Maxwell brought out his second famous paper on electro-magnetics, entitled "On Physical Lines of Force," there have been two major branches of modern physics, the one associated with Newton, Laplace, Dalton and Ampère and called particle physics, the other identified with Faraday, Maxwell, Larmor, Lorentz and Einstein and termed field physics. It has been one of the most grievous errors of philosophical thought and of scientific thought outside of theoretical physics to identify the whole of modern physics with nothing but particle theory.

The validity of causality in field physics as well as particle physics has been generally accepted. Both express their laws in terms of differential equations. Certainly there has been far more furor over the threat to causality in quantum mechanics than ever occurred in connection with field physics. Nevertheless, the first alteration in the definition of state at a given time, in connection with the retention of causality, was not made in quantum mechanics but as long ago as 1861 when Maxwell laid the foundations of modern electro-magnetic theory. Moreover, this new definition of state introduced by Maxwell and carried on by Einstein, is in certain respects, although not in all, a much more radical departure from the Newtonian definition of state than quantum mechanics makes and to this extent a much more serious threat to causality.

Nevertheless, causality holds in field physics as in particle physics and in quantum mechanics, for the result of Maxwell's new definition of state is not to invalidate causality but to designate more precisely the action of the past on the future in a peculiar way.

Again the relation of necessary connection between states at different times is retained. The difference is that it is defined by Maxwell's differential equations instead of Newton's or the Schrödinger time-equation, and the states which it relates are given in a unique way in each of the three cases.

We must turn, therefore, to the definition of state in field physics. The crux of the matter centers in what the physicists term retarded potentials. A potential, let it be recalled, is a function from which can be derived a direction and intensity of a force at a point in a field. A field conversely is a continuum of matter or of force, every point of which is defined by two factors differing usually from point to point, one of which designates the direction of the force at that point, the other of which specifies its intensity.

It might seem that the only alteration in the definition of state required to pass from particle to field physics would be the designation of the potentials, i.e., the direction-intensity quantities, at every point in the field of the system, instead of the positions

and momenta of all the particles of the system. This would be the case if field physics followed particle physics in assuming the action of forces instantaneously at a distance.

It is the rejection of this idea for that of force propagated from point to point in the field with a high but finite velocity which complicates the definition of state in field physics by requiring the introduction of retarded potentials. Only at the point in the field where the observer is located at the present time t_o do non-retarded potentials, i.e., directions and intensities of the forces at the time t_o, occur. At all other distant points the potentials which enter into the definition of state at the present time t_o are directions and intensities of the forces at those points, not at the time t_o but at an earlier time t_{-n} such that these forces, leaving the distant points at the time t_{-n} and traveling with the finite velocity c, arrive at the observer's position at the present time t_o. Such potentials applying only to points in the field distant from the observer and determined not for the present time t_o for which the state of the system is being defined but, nevertheless, entering into this definition even though determined for the past times t_{-n}, are called retarded potentials.

The reason for their use, be it noted, is that the rejection of action at a distance prevents the definition of state at a given present time t_o in terms of the instantaneous relation to each other of the forces at distant points in the field and requires its definition in terms of the forces at different points related to each other by a finite velocity. But to relate the force where the observer is located to forces at distant points in the field by a finite velocity is to prescribe that the potentials of the latter points for the state of a system at the observer's present time to must be those not at the time at which the finite velocity of propagation arrives at the observer's position but at the earlier time t-n at which it leaves the distant position. Hence, the need for retarded potentials in the definition of state in field physics.

Let us now consider the status and meaning of causality in field physics in the light of this definition. Causality holds in the sense that given a state at the present time t_o as thus defined in terms of retarded and non-retarded potentials, Maxwell's

equations in their earlier form, or in the invariant form provided by Einstein's special theory of relativity, permit one to deduce a determinate future state at the later time t_{+n} defined in precisely the same way. Providing that one permits each theory to define the state of a system at a given time in its own way, the relation of necessary connection between states at different times holds in field physics as it does in particle physics and in quantum mechanics.

If we concentrate attention, however, not upon the definition of state in field physics at the given time t_o, but upon the distinction within this definition between retarded and non-retarded potentials[3] then certain unique factors appear. First, there is no meaning in field physics, either theoretical or operational to simultaneity of action of the forces at all the points in the field at the present time t_o. To admit such a meaning would be to smuggle back the rejected notion of forces acting simultaneously at a distance. This entails that the causal action of the past with respect to the future is not with respect to the whole spatial field of force in the present for a given observer but merely of that point in the present where the observer is located. Thus, strictly speaking the causal action of the whole spatial field of nature in the temporarily extended past upon the whole spatial field of nature in the temporarily extended future is for a given observer with respect to but one point in the spatial field of force in the present, namely that point, designated by non-retarded potentials, where the observer is situated. It is to represent this peculiar relationship graphically that the familiar light cone illustration has been used.

This is well known. It was treated by Professor Weyl in 1926 in his paper before the Sixth International Congress of Philosophy at Harvard (4). It has received full popular exposition from Professor Eddington (5) and meticulous treatment by Professor Hans Reichenbach in his Philosophie der Raum-Zeit-Lehre (6).

I have returned to it here for different purposes. First, to demonstrate that the necessity of a radical modification of the

[3] The physicist is accustomed to treat a non-retarded potential as a special case of a retarded potential, namely, the case in which $n = o$.

definition of state in order to retain the concept of causality in physics did not originate in quantum mechanics but occurred as long ago as 1861 when Maxwell laid the foundation of electromagnetic theory. Second, to show that there is a respect in which the definition of state introduced by quantum mechanics marks a return to the definition of state used by Newtonian mechanics. This becomes clear when one fact is noted: Quantum mechanics does not prescribe the use of retarded potentials in its definition of state. This entails that it admit a meaning for the relation of every point in the spatial field of force of nature at any present instant of time and hence, return to the Newtonian concept of causal action of past upon future for a given observer by way of the whole spatial field of force of nature in the present.

It is precisely because of this return to the Newtonian concept of simultaneity for all points in the spatial field of force that ordinary quantum mechanics is at present incompatible with the relativity theory and that the resolution of this conflict is the basic problem confronting contemporary physics.[4]

The third reason for reexamining the definition of state in field physics arises because of its bearing upon causation in biology. This brings us to an aspect of the definition of state in field physics which we have not yet considered.

So far as biological systems are concerned the distinction between retarded and non-retarded potentials may be ignored, since the size of an organism is so small and the velocity of electromagnetic propagations is so large that for all theoretical and practical purposes it is as if we had action at a distance and only non-retarded potentials at every point of the field of the system. Consequently, for living organisms field physics differs from particle physics in its definition of state only in its substitution of force directions and intensities at every point in the field of the system for positions and momenta of all the particles of the system.

[4] To be sure Dirac has relativized quantum mechanics for the simple case of one electron, but not for general quantum theory which applies to atoms with many electrons. Moreover, some physicists do not regard Dirac's theory as satisfactory even for the simple case.

But in this little word 'only' there is a tremendous difference. This begins to become evident when one recalls that alterations in the definition of state at a given time represent differences in what the scientist finds it necessary to assume in the initial conditions in order to account for the subject-matter he is studying.

The restriction of the definition of state to positions and momenta means that any subject-matter for which particle physics is adequate is to be understood as the mere effect of the compounding of the motions of its constituent particles. Applied to a living organism this leads to the notion that its observed form and organization have no primary status or causal significance in themselves but are instead the mere effect of the motion of its atomic parts. In practice this produces a state of mind, very common among contemporary experimental biologists and physiologists, which regards any reference to the form of the organism as a whole as very superficial and which supposes that if the chemical components are determined all relational factors will be taken care of automatically as mere by-products.

The net result is that the central difficulty of biology,—the problem of organization, is ignored, or, if you prefer, solved by an arbitrary act of faith. Certainly no one has determined experimentally the positions and momenta of the trillions upon trillions of particles in even the smallest of living organisms. Nor would it do one very much good, so far as the deduction of biological organization is concerned, were all this empirical information obtained, for no mathematician has learned yet how to solve Newton's equations for three or four particles to say nothing about trillions upon trillions of them.

The position is little better with the modern alternative theories which emphasize the primacy of the formal factors by introducing such notions as holism, gestalt, emergent evolution or entelechy. Being largely intuitive and purely qualitative in character these concepts provide most excellent theories of biological causation when presented to the imaginations of mixed audiences in psychological and philosophical congresses, but unfortunately they turn out to be of little use when applied to the concrete bodies of the living creatures to which they are sup-

posed to refer. In the hands of the working biologist they lead nowhere except to futile debate.

These futilities will not be worthless, however, if they cause us to look elsewhere and remind ourselves that there are other conceptions of causation in physics than the one provided by particle physics. For the significance of the shift in the definition of state in passing from particle to field physics is that it completely reverses the status of relatedness and particles with respect to their primacy in the initial conditions which science must postulate to account for a given subject-matter. To put the matter over-simply but none the less correctly in general: whereas particle physics postulates the discrete atoms with their positions and momenta defined experimentally and then deduces the field and its relatedness, field physics postulates the field and its relatedness defined experimentally in terms of the directions and intensities of its forces at every point and then in part at least derives the local actions of ponderable masses.

The point, moreover, is not merely that physics has found this to be an alternative way of understanding nature, but that it is the only way if one would have a universe such as this one with interfering polarized light waves and the other electro-magnetic waves of the wireless in it. To put the matter mathematically, as Maxwell showed long ago, particle physics cannot derive the wave equation, and without the wave equation Young's and Fresnel's experimental proof of the interference and polarity of light and Hertz's discovery of the electro-magnetic waves other than light which Maxwell's field theory predicted, cannot exist.

The time has come, however, to allow Maxwell to speak for himself upon the relative status of relatedness and particle in field theory. It is to be remembered that the accepted theory of electricity and magnetism when Faraday (7) and Maxwell carried on their investigations was the rigorously mathematical one provided by particle physics. In his first paper in electromagnetics entitled "On Faraday's Lines of Force", Maxwell (8) says of Ampere's theory that its basic "assumption is no doubt warranted by the universal consent of men of science in treating of attractive forces considered as due to the mutual action of

particles." Then, in the same sentence, Maxwell adds, "but we . . . are proceeding on a different principle, and searching for the explanation of the phenomena, not in the currents alone but also in the surrounding medium."[5]

This medium he conceived, tentatively following Faraday, as a continuum of lines of force having their termini, if any, in what he called "sources" and "sinks". It is natural to identify these "sources" and "sinks" with particles of the system. It is to be noted, however, that the lines of force and the field are completely defined by the designation of the direction and intensity of the force at every point. Hence, any theory concerning the mechanism of the "sources" or the "sinks" or the medium is quite irrelevant for the precise formulation or experimental application of the theory. What is important is merely the relatedness of the forces throughout the field given directly by the direction intensity quantities.

This becomes explicit in Maxwell's third paper on "A Dynamical Theory of the Electro-magnetic Field" in which he drops the previous physical models and presents his famous equations. "We ought to be able," he writes, "to write out all the consequences of its [the field's] motion, provided we know the form of the relation between the motions of its parts." In other words, given the relatedness we can deduce the particle motions. Two paragraphs later he adds, "The phenomena of the induction of currents has been deduced from their mechanical actions by Helmholtz and Thomson. I have followed the reverse order, and deduced the mechanical action from the laws of induction," that is, from "the form of the relation between the motion of the parts."[6] As Professor Einstein has expressed it in a paper written upon the hundredth anniversary of Maxwell's birth (9), "It was clear that the equations themselves were all that was essential, and that the field intensities that appeared in them were elementary, not derivable from other simple entities."[7]

The relevance of this to biology is evident. Were field physics

[5] Scientific Papers, Vol. I, p. 193.
[6] Scientific Papers, Vol. I, p. 533.
[7] Pp. 70–71.

applicable to a living organism as a whole it would provide an experimentally determinable and quantitatively expressible definition of that "form of the relation between the motions of the parts" which is the organization of a living system. It would also designate the difference between this organic relatedness from species to species and individual to individual. In addition, a scientific basis would be provided for that theory of biological causation which conceive the form of the system as a whole as in part at least an irreducible causal factor, rather than a mere effect of the atomic constituents. This follows, be it noted, from the fact that the field intensities which define "the form of the relation between the motion of the parts" are, to use Professor Einstein's language again, "elementary, not derivable from other simpler entities," at least so far as field physics is concerned.

The crucial question, therefore, arises: Is field physics applicable to living organisms? Are they electromagnetic systems? Do they when examined with the experimental apparatus of a field physicist exhibit potentials at every point of their form as a whole? This question is not answered by the experimental work upon the so-called brain waves or upon action-current potentials in other parts of the body. Such evidence is suggestive but it demonstrates merely that certain parts of the organism at certain times have electrical concomitants; it does not show that the continuous field of the organism as a whole is characteristically and persistently an electro-dynamic field both spatially and temporally.

Five years ago in his experimental study of the reorganization of the developing nervous system in young growing Amblystoma, my Yale colleague Professor Burr noted phenomena which he found most natural to conceive in terms of field factors. The question immediately arose concerning their character and the hypothesis suggested itself to him that this field was physical and not purely biological in character (10). Meeting with little encouragement from his scientific colleagues, he consulted a philosopher. The reply was, among other things, that since particle theory has to be supplemented, at least, with field theory in physics, it would be exceedingly surprising if this were not the

case also in biology where the primacy of the relational factor is even more self-evident. Professor Burr suggested, therefore, that a joint paper be written in which the philosopher would designate the general considerations from physics and the philosophy of science, and the anatomist would indicate the more concrete factors from biology, which make it likely that living systems are physical systems in the sense prescribed by field physics. This paper, entitled "The Electro-dynamic Theory of Life" appeared in 1935 (11).

Meantime Burr attempted to secure the apparatus which would put this theory to a test. No available apparatus of the physicists was satisfactory, since one could not be sure that any potentials which appeared were not contributed by the apparatus rather than the organism. Finally, through the joint efforts of the anatomist and expert mechanic Burr, the physicist Lane, and the physical-chemist Nims, an apparatus called forth first by general scientific and philosophical theory, and shown to be possible theoretically by Maxwell's mathematical electro-magnetic theory, took on material form in fact in the Burr-Lane-Nims microvolt-meter (12). With this, one could be sure that any potential differences which were found belonged to the specimen.

In 1935 it was applied to the living organism (13). Since then tens of thousands of observations have been made on different forms of life from the egg of Amblystoma, a few millimeters in length, to the gross bodies of dogs and monkeys and men. Not only do the electrodes pick up potential differences on the organism as a whole, but also in decreasing intensity at a distance from the surface of the organism, equal in some cases to one-ninth of its length. Moreover, the patterns of these potential readings persist in time and remain remarkably constant through changes in many local chemical factors.

There are two strains of mice which to ordinary observation appear to be identical except that the one strain develops cancer when bred, in a very high percentage of the offspring and the other strain, only in an exceedingly small percentage. These two strains have different field patterns. This establishes the point suggested earlier in this paper that field physics provides not

merely an experimentally determinable definition of organization in general but also a criterion of the difference in the formal factor from one organism to another.

There are many other findings of great clinical and theoretical interest (14). I shall refer to but one. The exact time of ovulation in many different organisms, including the human, exhibits itself in a variation in the potential readings. This solves a problem upon which clinicans have been working for decades. It is also important theoretically. One of the unique characteristics of living things is that they reproduce themselves. The detection of ovulation by electrical methods shows, therefore, that an important part of one of the most distinctively biological processes is electro-dynamic in character.

From all these findings it may be concluded, consequently, that living creatures are electro-magnetic systems and that field physics applies to them. The Burr-Lane-Nims microvolt meter enables one to define the state of a living system at a given time t_o in terms of the force directions and intensities at every point in the surface field of the system. Certain of the conditions requisite for the validity of the concept of causality of field physics are satisfied. It follows, therefore, since field physics in its definition of state postulates the relational factor as in part, at the very least, irreducible, that biological causation must be viewed in the same way.

It may be added in closing that this opens up the possibility of applying Maxwell's equations to living organisms to "deduce the mechanical actions," thereby establishing a working connection between the field and the particle theories of living creatures (15). To this Professor Burr and the writer propose now to turn.

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Induction and the External World

BY

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MR. E. J. NELSON, in "The Inductive Argument for an External World,"[1] treats of fundamental topics with erudition and urbanity, but his essay remains inconclusive, I believe, with respect to its purpose of discrediting the argument. He agrees with Mr. Savery, Mr. Pratt, and me, as against the positivists, that the question of the existence of an external world is meaningful (249) and indeed of paramount importance for both metaphysics and logic. But he argues against us that it cannot be inductively established by its supposed power of "saving the appearances." He is not sure that any induction is valid; but even if other inductions are valid, he thinks this one can not be (240, 245, 247), for the following reasons. (1) By the multiplicative axiom of probability-theory, evidence for a hypothesis can only multiply its antecedent probability, and can increase the probability only when the latter is finite, while the hypothesis of an external world has and can have no finite antecedent probability (241, 245).[2] (2) By the same axiom, even granted an antecedent probability for the hypothesis itself, no experience can act as evidence to increase the probability of a hypothesis except in so far as the hypothesis implies, makes prob-

[1] *Philosophy of Science*, Vol. III, July, 1936, pp. 237–249. Numbers in parentheses in the body of the following text refer to pages of this essay.

[2] Mr. Nelson charitably surmises that the realists have not heard of the need for a finite antecedent probability (247). I cannot accept such absolution. Cf. "The Argument for Realism," *The Monist*, Vol. XLIV, 1934, p. 193.

able, or "explains" the evidence, and this the hypothesis of an external world cannot do (244, 245, 247).

I am not prepared to cavil at the multiplicative axiom. The axiom is not sacrosanct, however, and if its consequences prove inconsistent either with themselves or with sufficiently accredited belief as to what is probable and what not, we cannot admit that it is "explicative of the notion of probability" (241 n.).

It is not clear whether and how Mr. Nelson supposes any hypothesis to acquire a finite antecedent probability, and the same uncertainty attends Broad's and Keynes's treatment of the subject.[3] If the principle of antecedent probability allows *no* evidence to do more than aggrandize a probability already accrued from other evidence, how can there ever be any *first* evidence, and therefore *any* evidence, for any hypothesis? The only absolutely antecedent probability must be a literally *a priori* probability. The only promising source of such probability is the principle of indifference, according to which, prior to all evidence, any hypothesis has a probability of 1/2. Mr. Nelson rather brusquely dismisses this principle with a footnote, referring to a supposed proof by Keynes that it is not applicable where the alternatives are a hypothesis and its contradictory (246 n.) Keynes's critique of the principle[4] is based upon the fact that in such cases the two alternatives are not coordinate, but one is "subdivisible" into many alternatives, each coordinate with the other original alternative. This criticism, however, seems to depend essentially on an em-

[3] C. D. Broad, *Mind and Its Place in Nature*, and *Scientific Thought*; J. M. Keynes *A Treatise on Probability*, p. 238 ff. In ordinary cases the multiplicative axiom is used to appraise an increment of probability from evidence which is late and supplementary, and the antecedent probability of the hypothesis is its probability on the rest of the evidence, i.e., previous evidence. The realistic hypothesis seems to be in a different situation just because the evidence which the realist adduces for it is, so to speak, all the evidence in the world. In its case, therefore, we cannot overlook what we usually overlook in other cases, namely, that the axiom requires eventually a strictly *a priori* probability.

Incidentally, Keynes believes in the applicability of inductive method to the problem of the external world. Cf. *op. cit.*, p. 240. Throughout the following discussion I follow Mr. Nelson's example in taking Keynes's *Treatise* as standard reference. It is a good example, but we should remark that the whole question takes on a different aspect, not to the advantage of Mr. Nelson's argument, when considered in the frames of the probability-theories of von Mises, Popper, and Reichenbach, for instance.

[4] *Treatise*, p. 61, etc.

pirical circumstance: namely, that people ordinarily so frame their statements that a negative statement is compatible with more states of affairs than an affirmative, so that, *relative to this evidence*, it is correspondingly more probable that of two contradictory statements the negative statement is true. So long as we are inquiring into strictly *antecedent* probability, probability prior to *all* evidence, the principle of indifference is at least as obviously "explicative of the notion of probability" as the multiplicative axiom. On the other hand, if we are to take account of special knowledge to alter the incidence of the principle of indifference, we must take account of our special knowledge of the particular case in question. I profoundly doubt that the denial of the statement "An external world exists" is "divisible" into more coordinate alternatives than its affirmation. Even if it were, however, unless such divisibility were infinite, and infinitely greater than that of the affirmation, we could deduce from it only that the initial probability of the affirmation is less than 1/2, and not that it is infinitesimal.[5]

When Mr. Nelson traces ordinary antecedent probability to a "principle of induction" (241), he may mean that without some principle of limitation of independent variety,[6] there *are* an infinite number of alternatives to, and hence only an infinitesimal antecedent probability *for*, any hypothesis; and when he grudges this recourse to the realistic hypothesis (240, 245), he means that this principle of limitation (which is identical with the principle of induction?), obtains, if it obtains at all, only within the sphere of phenomena. We have already noted that it is hard to conjecture how the realistic hypothesis could have alternatives infinitely more numerous than its own species. But it is equally hard to conjecture how, if, as Mr. Nelson says, we as yet know virtually nothing about the principle of induction (239, 240, 242, 245, 248), we can know the singularly esoteric fact that the principle is not valid beyond phenomena. When Mr. Nelson reverses himself, and instead of confining the inductive principle to phenomena avers that, if it obtains at all, it cannot be merely

[5] Cf. Keynes, *op. cit.*, p. 238.
[6] Cf. Keynes, *op. cit.*, pp. 253 ff.

phenomenal, but must be transcendent and metaphysical (248), he would seem to renounce this whole line of argument.

Mr. Nelson asserts that his argument applies equally to the *denial* of an external world (246); that is, he supposes that the denial likewise lacks a finite antecedent probability. Such admission, as it stands, is a *reductio ad absurdum* of his argument, for it contradicts the law of excluded middle—that the probability of the logical sum of any hypothesis and its contradictory, on any evidence or none, is not infinitesimal but certainty.[7] If we overlook this technical lapse, we can read in Mr. Nelson's confession an admission at any rate that the *a priori* probabilities of the assertion and the denial of an external world are equal—which is, after all, an acceptance of the principle of indifference in its simplest form. To summarize our discussion so far: the principle of indifference appears to be necessary to assure a strictly prior probability to any hypothesis. If it is invalid in general, then no hypothesis has any probability. If it is valid for any hypothesis, there is no good reason to suppose it invalid for the realistic hypothesis. And there is good reason to suppose that it, or some sufficient variety of it, *is* valid.

Strictly speaking, we have said enough to vindicate a sufficient antecedent probability for the realistic hypothesis. Both Nelson (241) and Keynes,[8] however, somewhat obscurely claim *analogy* as an ordinary source of antecedent probability,—analogy with some generalization or other hypothesis already established. Now, analogy is an inductive process, and *a posteriori*. It cannot yield a strictly *prior* probability, but will also require a prior probability to make it effective. It is worth noting, however, that whatever force analogy can have toward founding any hypothesis, it can have toward founding the realistic hypothesis.

I believe that Mr. Nelson would deny the possibility of analogical support of the realistic hypothesis (244) on the ground that whereas other hypotheses are assertions about correlations of particulars having in common the character of phenomenality—the character of being experienced—the realistic hypothesis

[7] Keynes, *op. cit.*, p. 139, expresses the law: $a/h + \bar{a}/h = 1$.

[8] *Op. cit.*, pp. 239, 253.

makes assertions about a system of particulars which lack this character. But this circumstance does not really constitute an important logical disparity between the status of the realistic inference and that of any other. Every analogy useful for actual induction is imperfect. Every analogical inference ignores some characters which are both common and peculiar to one of the sets between which the analogy is supposed to hold, and every analogical argument runs the risk that these characters *may* be decisively important. A familiar type of cases where such common and peculiar characters are *known* to exist are those where we are inferring from events in the past to events in the future; but innumerable such common and peculiar characters exist, without our specific knowledge, in all instances of induction. As our knowledge grows, our judgments of relevance are sharpened, and we ignore such coincidences with more and more assurance. We are so sure of the irrelevance of *pastness* that, as Keynes remarks,[9] our certitude takes on the flavor of an innate truth. The irrelevance of *experiencedness* is equally obvious. Not the obviousness but the scientific success of such judgments of irrelevance should recommend them to us. In any case, the realistic hypothesis is once more no worse off than any other. All induction beyond instantaneous solipsism depends on ignoring the circumstance that it must consist in drawing analogies between *my present experience* and what is not my present experience.[10]

Mr. Nelson's second principal objection to realism as a hypothesis is that even if it did have a finite probability, this probability could not be increased by any amount of phenomenal evidence because such evidence cannot be really implied and explained by such a hypothesis. He supports this objection by two main arguments: that there can be no generalization covering both experienced and unexperienced particulars, as the law of gravitation covers both the perturbations of the orbit of Uranus and the mass and motion of the planet Neptune (244); and that

[9] *Op. cit.*, p. 256.

[10] Keynes makes virtually this point, when he defends the analogical argument to other minds by maintaining the irrelevance of the *myness* which is common and peculiar to one member of that analogy. *Op. cit.*, 258.

no allegation of an external world would explain phenomena unless a genuine causal efficacy, and not a mere correlation, can be established within that world and between that world and phenomena (239, 244, 248).

The argument for the claim that no generalization can be validated to cover both experienced and unexperienced particulars is in essence the same as the argument that there can be no analogy between experience and the unexperienced, and what we have said about the latter is applicable, in principle, to the former. There is always some character common and peculiar to the evidence upon which a generalization is based and yet not taken account of by the generalization. Experiencedness may be a tragically relevant character; but we have no reason to suppose that it is, and much reason to suppose that it is not, and if we are not justified in ignoring its phenomenal ubiquity, then we are not justified in any of our inductions.

It is difficult to construe Mr. Nelson's argument that if the hypothesis of an external world is to explain phenomena, there must be a real dynamic connection, and not a mere Humean correlation, within the external world and between that world and phenomena, because Mr. Nelson himself professes to believe, more or less (248), that there is such a world and such a dynamic connection. We cannot exploit this profession, however, because Mr. Nelson admits he has no reason for it (249), because he expressly disowns it at other points, and most of all because this whole type of argument is fundamentally mistaken. No such dynamic connection is essential to vindicate any hypothesis. No metaphysical dispensation, Mr. Lewis's *Mind and the World-Order* should have taught us, is either needful or helpful toward alleviating the inductive problem.

If I understand Mr. Nelson, he thinks that a positivistic description and prediction of correlations of characters may suffice for a knowledge of the phenomenal realm (239, 245, etc.), but that the explanatory nexus beyond experience would have to be a different sort of thing. Now, there may be room to doubt—although I do not doubt—the adequacy of mere factual correlations to enable inference from any one set of facts to any other;

but if they are adequate in one realm, they are adequate in another.

The explanatory situation is this. The characters of our experience display some trace of continuity and correlation—continuity of instances or events, and correlation of kinds or universals. From this fact issues our interest and our success in predicting one set of experiences from another. From the fact that experiences, more or less fragmentary, can under certain circumstances be systematically augmented by other experiences to appear less fragmentary and better continuous and correlated in something like a system, we develop the hypothesis of an external world: the hypothesis, that is, that *all* experience is embedded in a systematic matrix of reality most of which is not experienced. This matrix explains experience, not because of any mysterious dynamism inherent in it, but because it places the apparent vagaries of experience in an intelligible continuum. There presumably prevail in it not only a more complete and consistent correlation of characters than any one person's experience ever displays, but also a closer continuity from next to next in the dimensions of space and time. The latter property satisfies the fundamental principle of contiguous causality,[11] the modern version of the maxim that *Natura non facit saltum*, and it may be this which Mr. Nelson mistakes for a demand that the external world evince "real causality."

Mr. Nelson seems to feel that the realist has a perverse interest in the *unexperiencedness* of the great causal system. Such is not the case. The assumption of its existence, as a determinate continuum of characters, is what explains the phenomena. The assumption of its unexperiencedness, while perfectly compatible with the rest of the hypothesis, explains nothing except the empirical fact that we do not experience it. The unexperiencedness of most of the realist's world, the trait which the critic finds most noumenal and objectionable, is thus the one trait for which there is incontrovertible immediate evidence.

We could say of Mr. Nelson what Russell said of Bradley:

[11] Cf. V. F. Lenzen, "Physical Causality," e.g., *Univ. of California Publications in Philosophy*, Vol. XV, 1932, pp. 80–84.

that even if his argument were far stronger than it is, we should still be justly suspicious of a piece of subtle dialectic which contradicts a conviction more solidly rooted in us than any abstract principle of a still inchoate logical theory. Both Keynes and Nicod—whom Mr. Nelson himself has called to witness—suggest that an ultimate source of antecedent probability may be a primitive and unrationalized sentiment of plausibility[12] or verisimilitude.[13] If we needed to, we could claim for the realistic hypothesis, as for no other, the benefit of this.

We do not need such credential, however. Mr. Nelson has not shown that the disabilities which he imputes to the realistic argument really pertain to it more than to any other inductive argument; and he has not shown that they are real disabilities. And although Mr. Nelson's argument is valid only if he can demonstrate with complete certainty that realism has no finite antecedent probability, our argument is conclusive if it only establishes that realism *may have* a finite antecedent probability. For, as Keynes points out,[14] if it is finitely probable that a given hypothesis is finitely probable, then that hypothesis *is* finitely probable.

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[12] J. Nicod, *Le Problème Logique de l'Induction*, p. 19 n.
[13] Keynes, *op. cit.*, 239, 247 n.
[14] *Op. cit.*, pp. 259–260.

The Concept of Law in the Social Sciences[1]

BY

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IT IS the thesis of this paper that the term scientific law can and should mean in the social sciences exactly what it means in any of the other sciences. There seems to be considerable agreement among scientists as well as others that a scientific law is a generalized and verifiable statement, within measurable degrees of accuracy, of how certain events occur under stated conditions. If I were to attempt a more specific statement I would say that a law is (1) a group of verbal or mathematical symbols (2) designating an unlimited number of defined events in terms of a limited number of reactions (3) so that the performance of specified operations always yields predictable results within measurable limits.

It will be observed that the definition I have given contains three distinct requirements, to wit: (1) a generalized statement of some behavior sequence; (2) a statement of the conditions under which the generalization is verifiably true; and (3) a statement of the degree to which it is verifiably true under these conditions. Most generalizations in the social sciences fulfil as yet only the first of these requirements. What we do not know is (1) under what conditions these generalizations are true, and

[1] Read at the Second Conference on Methods in Philosophy and Science, New York City, November 28, 1937.

(2) to what degree they are verifiably true under these conditions, i.e. their probability.

Is it possible to formulate generalizations about social phenomena which will meet all of these requirements? Some hold that this is impossible, and that scientific laws of social behavior are therefore also impossible. A recent paper[2] in *The Philosophy of Science* has espoused categorically this position. Since it raises all the familiar arguments, and since it appears in a journal of repute, I shall use it as the point of reference for the presentation of a diametrically opposite position.

It appears from the paper referred to that not only scientific laws as defined above but objectivity itself is impossible in the social studies for the following principal reasons:

1. The phenomena of the social sciences are subjective. "Today, sensations and emotions and values are the phenomena about which the social scientists would like to be scientific."[3] In aiming at social laws of the type here defined or indeed at objectivity of the type found in the physical sciences, "social scientists thereby set for themselves the paradoxical ambition to be objective about the subjective aspects of life."[4]

This objection is based upon a postulate which, if admitted, undoubtedly ends once and for all any question of either objectivity or laws in the social sciences. According to this view, what are objective and what are subjective aspects of phenomena is apparently given in the data. They were apparently clearly so labeled by the Creator. It happens that the social sciences are concerned with those labeled subjective. Ergo, objectivity, not to mention laws, is impossible in the social sciences! Q. E. D.

It should be unnecessary to point out that no science can accept the postulate that objectivity and subjectivity are qualities given in data rather than being our way of responding to the data and communicating such responses. What are, on the contrary, the postulates of science regarding the nature of all data?

[2] E. Bisbee, "Objectivity in the Social Sciences", *Philosophy of Science*, July 1937, pp. 371–82.

[3] *Ibid.*, p. 372.

[4] *Ibid.*, p. 372.

This question is fundamental to the principal issue agitating social scientists today, and I know of no way of approaching this question as to the possibility of social laws except through a review of the postulates underlying all science and therefore underlying the notion of scientific laws. I believe that the following are postulates[5] which must be assumed by all natural scientists, whether in the physical, biological, or social sciences:

1. All data or experience are *the responses of organisms-in-environment.* This includes the postulate of an external world and variations both in it and the responders to it.

2. Symbols, usually verbal, are invented to represent these responses.

3. These symbols are the immediate data of all sciences and of all communicable knowledge.

4. All propositions or postulates regarding the "ultimate realities" must always consist of extrapolations, generalizations or abstractions from these symbols and the responses which they represent.

5. These generalizations or extrapolations are in turn represented symbolically, and we respond to them as we respond to other phenomena which evoke behavior.

Some of the corollaries and implications of these postulates, especially as they affect present methods in the social sciences, need to be stated in this connection.

1. In the first place, under these postulates, all statements about the nature of the universe or any part of it are necessarily a verbalization of somebody's responses to *that which* evoked these responses. The nature of that which evoked them must always be an inference from the immediate datum, namely, our sensory experience. All assertions about the "more *ultimate* reality", "nature", "essence", or "being" of "things", "objects" or "experience" are therefore unverifiable hypotheses, and hence outside the sphere of science. Conversely, we must assume, if we want to operate within the scientific framework, that man and culture are definitely part of the cosmos, and that, in common

[5] For a useful distinction between axioms and postulates, see E. V. Huntington, "The Method of Postulates," *Philosophy of Science*, Oct. 1937, p. 484.

with non-human phenomena, all phenomena of man and culture are entirely contained within the physical cosmos and entirely dependent upon the energy transformations within that cosmos. We start with human responses as the immediate datum. As a metaphysical necessity we grant *that which* in the universe outside or inside of the responding mechanism precipitates the response. But beyond this it matters not at all to the scientific universe of discourse which of any alternative hypotheses anyone may prefer to hold about the more ultimate nature of *that which* arouses responses.

2. It follows from the above that for scientific purposes all attempted distinctions, hypotheses, or assumptions regarding differences in the *ultimate* "nature" of so-called "physical" as contrasted with "social" data, between "material" and "immaterial", "mental", "spiritual" or "cultural" phenomena are ruled out because if they exist at all for science they must consist of human responses that can be communicated. The data themselves are not ruled out insofar as they are manifest in human behavior of *any observable kind.* Since objectivity is here regarded *not as a characteristic of things but as those ways of responding which can be corroborated by others*, it follows that the framework of science, just as fully as any philosophical or theological system, affords place for all known or knowable data. Of course, the less developed our objectifying techniques for certain experiences (i.e. the "subjective", "spiritual"), the greater is the task of objectifying them sufficiently to communicate them. Indeed, this process of objectifying them may involve analysis, reclassification, and designation by new and strange symbols. Many terms at present employed probably will be abandoned entirely as devoid of content when the behavior phenomena to which they once referred will have been more adequately described by other terms. This has been true of all prescientific terms and categories. In this connection, we encounter one of the chief obstacles in the translation of subjective experience into objective data. Let us take only one illustration.

In the opinion of the best chemists, as recently as 150 years ago, phlogiston was a necessary element in the explanation of

combustion. The theory was that in all materials that burn there is present phlogiston, a substance without color, odor, taste, or weight. Even Priestly, to whom the discovery of oxygen is usually credited, continued to maintain during his lifetime the existence of phlogiston and the part it was supposed to play in combustion. By experiments involving much careful and accurate weighing, Lavoisier was able to demonstrate finally the unnecessary character of the hypothetical entity, phlogiston. Nevertheless, the older chemists of the day, thoroughly habituated to thinking about fire in terms of phlogiston continued to "feel" that the new explanation "left something out." It did leave something out, namely, a *word* to which the chemists of the day had become thoroughly habituated, and which was therefore as "real" to them as the word "wood" or whatever other words are used to symbolize the factors assumed to be present in a given fire. But we do not contend to-day that by abandoning phlogiston modern chemists refuse to recognize a vital or relevant element in the explanation of fire.

To-day, however, many students of social phenomena are firmly convinced that the phenomena with which they have to deal cannot be adequately described or explained without, for example, a category called "mind", which carries with it a whole vocabulary of subsidiary terms such as thought, experience, feeling, judgment, choice, will, value, etc., etc., by which they avoid the necessity of defining operationally the behavior-units into which the phenomena of any field must for scientific purposes be divided. Any attempt to deal in other terms with the behavior which these words are used to represent meets with the most determined resistance on the ground that "something has been left out". And what has been left out? Why, "will", "feeling", "ends", "motives", "values".[6] These are the phlogis-

[6] For example, Morris Cohen's categorical statement, "Psychic forces are not physical forces." *Reason and Nature*, p. 360. The difficulty is further illustrated by the preposterous accusation that those who find the framework and methods of the other natural sciences also adequate for the social sciences deny the relevance of anticipated social ends as a partial determinant of social action. (See R. K. Merton, "Durkheim's Division of Labor in Society", *American Journal of Sociology*, November 1934, p. 321. "Anticipated ends", whenever they are incentives to action exist in the form of words or other symbols,

ton of the social sciences. Argument or demonstration that the behavior represented by these words is accorded full recognition within the present framework of the natural sciences are to some apparently as futile as were the arguments against phlogiston to Priestly. He "*just knew*" that any system which left out the word phlogiston was *ipso facto* fallacious. I have not the slightest doubt that a considerable part of the present content of the social sciences will turn out to be pure phlogiston. That fact will be discovered as soon as someone attempts operational definitions of the vocabulary which at present confounds these sciences.[7]

Within the scientific universe of discourse, then, all data are known to us only through human responses and we infer both the existence and the characteristics of any data from these responses. A taboo, a custom, an "idea", or a belief is, as data, as "tangible", "real", observable, measurable, and otherwise susceptible to scientific study as is a stone, a table, or a horse. The scientific data concerning these entities consist in all cases of our symbolized responses to them. When these responses are verifiable and are ordered in a certain way or ways, we say the data are objective and measurable. The terms "tangibility", "reality", etc., may be used profitably to describe a degree of objectification of our responses to some data while such terms as "intangible", "spiritual", "nonmaterial", may be used to describe a lesser degree of objectivity of responses. But these terms cannot be used to indicate intrinsic characteristics of data in the present frame of reference. The alleged greater "tangibility" of certain "physical" events resides not in the events but in our more

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to which the organism responds as it does to other stimuli. The same is true of memories, "values", "meanings", "ideals", "ideas", and all the rest of the phenomena which are alleged to be unique in the social sciences.

[7] For a criticism of operationalism see R. B. Lindsay," A Critique of Operationalism in Physics", *Philosophy of Science*, October 1937, pp. 456–470. The criticism is really not of operationalism so much as of *sole* reliance on it. It is doubtless true that until strictly operational concepts are developed, non-operational concepts may be useful. This concession may be made without detracting in any way from the desirability of operationalism as a goal to be sought. The operationalist viewpoint is well stated in P. W. Bridgman, *The Logic of Modern Physics* (Macmillan 1932). Also A. F. Bentley, *Linguistic Analysis of Mathematics*, ch. 13. (Principia Press, Bloomington, 1935.)

highly objectified methods of responding to them. *That response* which we call custom, affection, pain, anger, or what not, is experienced through sense receptors from outside or inside the organism as truly as our experience of a stone or a tree.

2. The second objection to the possibility of scientific laws in the social sciences, urged in the paper under criticism, is that social data lack sufficient homogeneity to make it possible to formulate generally valid statements about them.[8]

This difficulty is practically the same as that considered above. The first objection to objectivity in the social sciences, it will be remembered, was that the data of the social sciences *are* subjective. Here the objection is that these data *are* non-homogeneous, (or that they *are* "complex"). Again, if one grants the postulate that homogeneity is an intrinsic characteristic of data rather than a word describing the degree of care with which man has classified and categorized his responses (i.e. responded selectively), the conclusion doubtless follows. But from the postulates from which science proceeds, as stated above, non-homogeneity is merely the measure of how inadequately we have classified data. The homogeneity of H_2O resides precisely in the abstraction of these elements, as contrasted with the full heterogeneity of all the liquids that are called water in daily discourse. The heterogeneity of social data at present resides in the relative crudeness of the categories we employ to delimit and designate social observations.

3. Finally it is urged in the paper cited that social laws are out of the question because it is impossible "to exclude all irrelevant factors and to control all relevant factors"[9] in studying social phenomena.

[8] Bisbee, *Op. Cit.* p. 374. For a detailed refutation of the whole viewpoint here under criticism see Read Bain "The Concept of Complexity in Sociology" *Social Forces.* Dec. 1929 and Mar. 1930. Also G. A. Lundberg, "Is Sociology Too Scientific?" *Sociologus* Sept. 1933. Also "The Nature of Sociological Laws", *Jr. of Social Philosophy* (forthcoming).

[9] *Ibid.* p. 378. In illustrating this point the author uses several examples which betray in a striking manner the superficiality of her analysis, as far as proving the impossibility

This objection comes near to stating the principal shortcoming of the social sciences today, namely, their inability to measure and therefore to "control" the various conditions that influence social behavior. There are two ways in which this control can be achieved; (1) By laboratory elimination of the disturbing influences (e.g. vacuum chambers, glasses eliminating certain light rays, etc.) or (2) by measuring these influences so that allowance can be made for their influence in the observed results. The reservation of "other things being equal" is not the serious drawback which the above author assumes, nor is it unique in the social sciences. All scientific laws stipulate under what conditions the reported sequence occurs and implicitly or explicitly the degree of probability of their occurrence under these conditions. The really serious difference between this situation and that which obtains in the laws of social science today is that the latter do not know *what* conditions and *what* degrees thereof they include under the reservation "other things equal". Only when these conditions are known and measured do we have a scientific law as defined in this paper.

It is by virture of this knowledge that the other sciences can control, or what amounts to the same thing, allow for, the operation of these conditions, both in the laboratory and especially in the practical applications of these laws toward the objects of human striving. The control of altitude and air pressure in a demonstration of the law of falling bodies comes only after we know that these factors influence the behavior in question. The usefulness of this law, for example in the calculation of the path

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of social laws is concerned. That water freezes at 32°F is held up as an example of the invariant validity of the laws of physics as a result of the stability of their subject matter. What would become of the reliability of this law if water were not first defined as H_2O instead of including under that word all fluids that our unaided senses identify as water, and if instead of a thermometer's reading of temperatures, the subjective estimates of different individuals were accepted? "Water" would under these conditions, "freeze" under such apparently varied "temperatures" as to make this law as unreliable as social laws appear to be. See also this author's amazing illustrations of the alleged shortcomings of the "law" of supply and demand and of Cooley's "laws" of self-estimation. It should be unnecessary to point out that the conclusions drawn flow entirely from a postulate of complete "free will" in man as contrasted with the rest of nature and not from any intrinsic nature of social situations.

of a projectile or other practical use, depends upon our ability to *measure to what extent* these conditions in a given practical situation influence the event. Without the knowledge of what conditions influence the behavior described by a law the necessary laboratory conditions cannot be set up and the law cannot be formulated. Without methods of measuring to what degree conditions in actual practical situations deviate from those laboratory conditions the law is useless for practical purposes. Actually, of course, the knowledge of the conditions that influence the behavior described by the law, and the control or allowance for them, constitutes a kind of measurement. The most conspicuous defect of most present "social laws" is, as stated before, precisely this: *We do not know under what specific conditions they are true and to what degree they are verifiably true under these conditions.* This defect can be remedied in social as in physical science only by the development of techniques of sociological measurement.

To this proposition most of the skeptics regarding the possibility of social laws might agree, because they would take it as only additional proof of their main contention. All they have to do (in addition to declaring social phenomena subjective and non-homogeneous by nature), is to declare them also to be by nature non-measurable.[10] If the condition here admitted to be essential, namely, the measurement of social forces, is impossible, then it follows that the pursuit of social laws is futile. On the other hand, if such measurement is possible, then the path of the social sciences leads across the same difficult but not insurmountable terrain over which physics and other sciences have travelled to their present conspicuous triumphs. The possibility of formulating reliable laws in the social sciences must be admitted

[10] This appears to be implied, for example, in such a statement as the following: "In the case of most of the psychosocial qualities this quantitative description has been and will probably remain useless, partly because these qualities *do not have* any unit of quantitative measurement, partly because the fundamental categories which compose the framework of reference in the social and cultural sciences *are* predominantly qualitative." [Italics mine]—P. A. Sorokin, *Social and Cultural Dynamics*, Vol. 1, p. 159. Elsewhere (Vol. II, pp. 21–22) this author expresses views quite compatible with mine as to the permissibility of quantifying formally gradations now expressed only in terms of "more" or "less".

to depend mainly upon the possibility of developing reliable techniques of measuring social conditions and forces.

I believe the possibility of developing such measuring techniques exists and that considerable progress in this direction has already been made. I have elsewhere[11] set forth in some detail the theoretical grounds for this belief, and space forbids an adequate review of this position, as well as of the concrete advances made in the development of sociological measurement.[12] I may conclude, however, with a brief summary of the reasoning involved, especially since the postulates upon which it is based have already been stated above. For a more thorough exposition of the case, I refer to the source cited above.

First among the obstacles to the development of measurement in the social sciences is the assumption that measurability, like objectivity and subjectivity, is a quality inherent in data rather than a way of responding to phenomena. Thus it is cavalierly declared that this or that class of phenomena are not measurable. What is meant is that scales or other devices for the more accurate and objective measurement of these data have not yet been developed. I have already pointed out that for scientific purposes the qualities which we attribute to data are only words with which we describe our reactions to situations. Science made progress when it ceased to confuse words with the things they signify and began to regard definition and measurement not as mysterious intuitive revelations but merely orderly and selective ways of responding to situations. In the same way science has abandoned a postulate still highly regarded in the social studies, namely that before a thing can be measured it must first be defined in folk-words supposed to possess some final essence. Instead, scientists have taken the position that force *is* that which makes pointers move across dials, time *is* that which clocks measure, and space *is* that which we measure with yardsticks. Consider the wearisome tomes that were produced in times past

[11] Lundberg, G. A. "The Thoughtways of Contemporary Sociology", *American Sociological Review*, October 1936.

[12] References to some of these will be found in my paper "Quantitative Methods in Social Psychology", *American Sociological Review*, Feb. 1936, pp. 50–51.

attempting to define in terms of more final essence the "nature" of time, space, or force. These works strongly resemble the more ponderous sociological and economic treatises of our day. Social scientists will be less worried over the impossibility of measurement in their field when they once realize that the definition of a phenomenon and its measurement is in science the same operation; that it consists of agreeing upon sufficiently definite symbols with which to designate *that which* evokes a certain type of human response.

The serious and difficult task of developing the operational conceptual systems and technical devices which constitute measuring instruments in every field remains for the most part yet to be accomplished in the social sciences. The labors by which ammeters and microphotometers were developed cannot be avoided if we want social laws comparable to those of chemistry and physics. There are no logical or *prima facie* grounds upon which the development of comparable instruments for measuring social phenomena can be declared impossible. The ultimate test of the validity of this position will consist, of course, in the invention of such measuring devices and the demonstration of their validity by exactly the same criteria by which measuring devices in the physical sciences are validated. The last twenty-five years has, in my opinion, produced striking advances in this direction in the fields of psychology, sociology, and economics.

There are those who regard the movement as a passing fad and who even profess to see signs of its passing. They are entitled to their opinion but I am not impressed by the evidence, if any, which they advance in support of their prediction. It dangerously resembles these wishful prospects we tend to see when our traditional equipment becomes badly adapted to some new turn of events. The problem of more accurate sociological measurements will recur as long as men have to adjust to a changing world. Broadly speaking it even may be said that, the inadequacies of our adjustments in the social world are a fair index of the crudeness of our measurements, for inaccurate appraisal of social situations is everywhere apparent. In the meantime, the theoretical rationale here presented is demanded both as an

hypothesis on which to work and as a justification and an incentive for devoting ourselves to the slow and undramatic labor upon technology which always has been one of the indispensable conditions of scientific progress in all fields. Finally, as long as the physical sciences retain and constantly expand their dominating position in other aspects of life, it will be impossible to avoid the implications of their methods for the social sciences. Whatever the final results of attempts at increasingly wide and accurate measurement in the social sciences may be, we are more likely to make progress in that direction, which everyone agrees is desirable, if we proceed on the hypothesis that it can be done.

SUMMARY

I have attempted to show in this paper that the apparent difficulty of applying the methods of natural science to sociological phenomena flow not from any intrinsic characteristics of sociological data but from the retention in the latter field of postulates long since repudiated in science. Indeed, the postulate of such intrinsic difference as regards man's method of knowing about the two fields of human observation is the principal assumption which must be abandoned if the social studies are ever to become sciences in the sense that physics or chemistry are sciences.

Since Darwin, the abandonment of that postulate has been reluctant but steady. The abandonment of a time-honored postulate and the adoption of a new frame of reference frequently calls for cruel partings with eloquent vocabulary, learned lecture notes, and vested intellectual interests. We try to achieve the transition as gradually as possible in order not to shock the organism too violently by the change. The technique of changing one's postulates usually is to quit talking about postulates entirely until one has developed some verbal facility in the new orientation. Social scientists are in this transition at present. The ways of science attract us strongly both because of the results that can be achieved with science and because of its academic and public prestige. But we also like to retain some beloved articles of our erstwhile faith. The latter frequently embarrasses us, so we try to hide it as much as possible, but continue to

use it until we can acquire equipment suitable to the new orientation.[13]

Social scientists are at present engaged in the old attempt to eat their cake and have it too—to be scientific but not to learn any mathematics, to generalize but not to acquire the only technique by which masses of data can be legitimately generalized. They have never explicitly stated the fundamental postulates upon which they proceed and for the most part have refused to face those which indubitably underly the whole scientific approach. In short, they are engaged in trying to bootleg their non-scientific concepts and frameworks into the law-abiding domain of science.

The postulate that some data are inherently and intrinsically objective or subjective, homogeneous or non-homogeneous, measurable or non-measurable, is in some orientations entirely defensible. The scientific orientation proceeds, however, on a contrary hypothesis, namely, that these categories represent not intrinsic characteristics of data, but different ways of responding to data. All known data have this important characteristic in common: they become known through human reactions. I hold that by certain methods, through certain inventable operations, the subjective becomes objective, the non-homogeneous becomes homogenous, the "complex" becomes "simple", and the non-measurable becomes measurable.

From this point of view, I find Margenau's statement in another connection the inevitable conclusion. He says:

"If the fundamental thesis of quantum mechanics is correct, *all* physical observations form a probability aggregate, that is, a set of data to which the probability calculus can be applied. . . . And if, as is frequently supposed, the data of all exact empirical sciences are basically physical, we are confronted with the possibility that the entire physical world, [in which I include the social—G. L.] ultimately resolves itself into a set of events joined merely by the rules of the probability calculus."[14]

[13] V. Pareto has analyzed this process in terms of residues, derivations, and non-logical thoughtways. See his *The Mind and Society* (Harcourt Brace 1935). Vol. 1.

[14] *Journal of the American Statistical Association*, March 1936, p. 27. Discussion by Henry Margenau of paper by E. Nagel "The Meaning of Probability".

If this is the nature of contemporary science, then I think the methods by which scientific laws are to be achieved in the social sciences were correctly summarized more than twenty-five years ago by Henry L. Moore in his *Laws of Wages*, (pp. 4–5) when he said:

"The problems of natural science have required the invention of a calculus of mass phenomena that will probably yield its best results when applied to the material of the social sciences. The wealth of the statistical material . . . is itself a source of embarrassment. To utilize it for scientific purposes, it must be described in brief, summary formulae, and these formulae must be arranged upon a plan of increasing complexity so that it will be possible to pass from accurate descriptions of mass aggregates to the relations between the aggregates themseves."

Some feel that this view contemplates "forcing" the social sciences into the terminology and methods of physics, and that on the contrary every science must adopt the concepts and methods "adapted to its own subject matter". The ends of science are presumably the same in all fields, namely, to arrive at verifiable generalizations as to the sequences of events. Any methods that yield these results are permitted in all the sciences. It happens, however, that generalization implies applicability to *numbers of cases*, and hence all scientific laws, including those arrived at by so-called qualitative analysis, ultimately rely for their scientific validity upon the fact that they can be, and have been, *corroborated in numbers of cases*. The trend toward the adoption of mathematical techniques as any science matures is, therefore, not due to any compulsion to "imitate physics". The compulsion flows from the nature of accurate generalization, which is the essence of science. It is fallacious, furthermore, to assume that the symbolic devises (language) of contemporary physics is in some way inherent in the subject matter of that science. This assumption is merely another case of confusing language with things represented by language. The terms used in physics or any other science are basically definable only as names of *behavior-relationships* among phenomena and not names of any specific *objects*.

The notion is, unfortunately, still very wide-spread that such terms as, for example, atom, electron, quanta, energy, force, etc., are small pieces of rock or explosives—*matter* of some kind—instead of merely words representing convenient observation units of behavior. In short, if the word *atom* in the English language means an elementary behavior-unit of observation, it may, if similarly defined, be used with equal propriety in sociology as in physics. The fact that the calculus has thus far been employed chiefly in physics surely does not mean that its utilization in regard to other subject matter constitutes an attempt to "force the social sciences into the framework of physics." If in the course of scientific advance generalizations are found which apply with equal validity to "physical" and to sociological situations, this is, under the principle of parsimony, entirely desirable. To object to such generalizations on the ground that they applied to both "fields" would surely be the extreme of scientific provincialism.

In short, we adopt no concepts or mathematical techniques *because* they are used in other sciences. We adopt them, if at all, because they are effective tools in reaching admittedly desired ends. The latter criterion also allows full scope for the invention by social scientists of new symbolic systems or procedures uniquely adapted to their special problems. The postulate of the ultimate unity of all science is doubtless implicit in the principle of parsimony. But this principle may just as readily call for modifications in the physical sciences as in the social, if the principles of the former are found too narrow to encompass the latter.

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The Meanings of Culture

BY

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FEW words appear more frequently in the literature of contemporary social science than "culture." Not only does it occur frequently, but with such a multitude of meanings that a single definition of the term becomes almost impossible. Even a cursory examination of social scientific writing discloses that its signification varies with the contexts in which it appears and with most individuals who use it. As long as words are single exhibits of the versatility and variety of language, no fault can be found with a plurality of meanings which may come to adhere to them. But when words are designed for and used in discussions of a supposedly technical character in any of the social science disciplines, the time for literary and philological speculation has past. Words intended for such use must conform to certain standards of conceptual validity, must be used consistently in analogous contexts, and must never be torn from the universe of discourse of which they are a part.

Unfortunately, the rapidly accumulating writings of anthropologists, sociologists, social psychologists, and historians reveal that the use of "culture" seldom observes these necessities. Instead of attaining to the dignity of a scientific concept, the loose and inaccurate way in which the word is used threatens to postpone longer the time when it shall attain to such dignity. Inconsistency of itself would be no serious fault if the separate meanings remained clear, but the inexact way in which "culture"

appears often vitiates discussions intended to display the nature of social change, the influence of social groups on each other, and the fundamental relations between the individual and the society to which he belongs. If the word is to become a scientific concept, if it is to lead to valid conclusions concerning the nature of social change, if it is to be helpful at all in the study of society, the meanings should be clarified by a diligent exegesis. If the logical positivists have insisted upon a clarification of the concepts used in the natural sciences, it is no less imperative that a thorough-going analysis of the terms used in the social sciences be undertaken, not only that their meanings become more consistent, and their use more fruitful, but that problems hidden under obscure and careless terminologies be brought into the light.

Exegesis of any word involves a comparison with certain criteria of conceptual validity. "Culture," for instance, in order to become an instrument of precision in social science, must first be clearly and distinctly defined. Second, its extent must be limited to a definite range; in other words, it must neither be too particular nor too general, for the intension of a concept varies inversely with the extension. In the third place, it may be agreed without invoking a prolix discussion of utilitarianism or pragmatism that a concept, to be a concept, must be useful. These are the criteria, then, by which we may attempt to measure the concept of culture.

Four interpretations stand out from the rest as rubrics under which practically all illustrations of the use of "culture" can be subsumed. They include (1) culture as the veneer of refinement, taste, and comity which covers the most jagged surfaces of our barbarian ancestry; (2) the higher expressions of group life, such as art, religion, science, literature, and philosophy; (3) all forms of group life, comprising institutions, artifacts, mores, customs, rites, ceremonies, and behavior patterns; and (4) the organic unity or dynamic ethos of a social group in its growth and development. Four characteristic definitions correspond to these four interpretations. Representing the first, A. Lawrence Lowell conceives of culture as involving "enjoyment of the things the world has agreed are beautiful; interest in the knowledge that mankind has

found valuable; comprehension of the principles that the race has accepted as true."[1] In the second sense Robert M. MacIver's statement may serve, that "culture includes the church and all the associations designed to foster art or literature, or drama or pure science or philosophy or any mode of thought or of life, of spiritual or aesthetic fellowship or of sport and recreation."[2] E. B. Tylor defines the word in its third connotation: "Culture is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society."[3] For the fourth meaning we may use a definition listed by Wilson D. Wallis, "A culture is a functioning dynamic unit, and the various traits which compose it are interdependent."[4] Of the definitions given to the word by writers in all disciplines these seem best to portray the meanings of culture.

When inquiring into the methodological value for social science of each of these interpretations it becomes apparent that the first has none at all. Although the number of individuals possessing personal refinement in a given social group may be an index of the status of that group in relation to others, on the whole this type of culture is so relative as to lose all scientific utility for the study of society. Passing, therefore, to the second —culture as meaning those higher life accomplishments of particular societies—we note that this use corresponds to the preceding personal sense of the term, but with changed extent. Now we are considering not the learning and refinement of an individual, but those so-called higher accomplishments of a group, those manifestations of poetry, music, graphic and plastic arts, religion, science, codified law, and philosophy which become an

[1] A. Lawrence Lowell, from "Culture." An address prepared for delivery at the University of North Carolina, April 28, 1915, but not delivered at that time. Reprinted in *At War with Academic Traditions in America*, Harvard University Press, 1934, p. 117.

[2] R. M. MacIver, *Society: Its Structure and Changes*. New York: Ray Long and Richard Smith, 1931, p. 226.

[3] E. B. Tylor, *Primitive Culture*, Volume I. London: John Murray, Albemarle Street, 1872, p. 1.

[4] W. D. Wallis, *Culture and Progress*. New York: Whittlesey House, McGraw-Hill, 1930, p. 11.

integral part of group life. This use neither need detain us long. For most practical purposes the culture concept in this sense presents only two methodological possibilities. First, it assumes importance whenever ideologies become prime as determinants of social change, when a doctrine or a theory or a work of art becomes so significant as to become itself a social force. It may then help formally to integrate the intellectual motivations of social change and facilitate discussions of the movements of ideologies from one social group to another. Second, the concept may become an heuristic principle for the division of world history into convenient periods for study. The period in which a certain type of art predominates, for instance, may be called the Baroque culture; another, in which a different type supersedes, the Rococo —expressions which are then applied to other phases and correlative aspects of life. Any epoch which can be interpreted in terms of the dominance of a congeries of homogeneous cultural factors, in this high artistic and religious sense of the word, may be called a culture itself. By means of such delineation it can be compared as to duration, extent, comparative influence, and intellectual achievement with other epochs in world history which may be built or drawn around similar configurations of homogeneous phenomena Thus, this type of culture may be defined in a relatively clear fashion as comprising the art, science, religion, literature, and philosophy which a group of people happens to possess at a given time in history. In extent, any group of people creating, developing, or participating in distinct types of these achievements may be said to share a common culture. Concerning utility, the concept may help sociologists to measure the influence of ideologies upon social change, to delineate the creative and cognitive aspects of group life, and to compare intellectual achievements in the light of the milieu from which they spring.

Culture in the third sense, defined by Tylor as "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society," differs radically from the two previous in spite of superficial similarities. It comprises not only those higher and more refined attributes of individual and group life, but everything

which accrues to people by their habit of living together. All artifacts are culture—stone jugs, eating utensils, weapons for killing animals, and implements for procuring and preparing food —whether or not they possess any utility over and above the perpetuation of existence, that is, whether or not they belong to the category of artistic productions. This type of culture also comprehends laws, customs, institutions, mores and morals, ceremonies and rites—all common ways of doing and thinking. It includes those things which, shared and mutually exercised, distinguish man as a member of a social group. Here all things, material, institutional, custumal, become not a part of culture, but culture itself. Together, as a complex whole, they are culture.

Professor Tylor's definition appears to be clear and distinct. In actual use, however, it has been strained far beyond his original intention. This type of culture cannot, by definition, be developmental nor dynamic. A complex whole of traits has never yet acted nor reacted, nor has it moved through cycles, spirals, or helices of development. It simply is. One may correctly describe it as social heritage, passed on from generation to generation, and swelled by constant accretions of new materials and customs along the way. The development of individual traits, such as marriage ceremonies or bone knives, can possibly be charted according to a pattern, but culture itself, as a generic, singular term indicating what man has, is a dead mass, a simple collection. To make it the subject of an active verb of motion does violence to the meaning of the word. When writers remark that this type of culture may be regarded as a functional, dynamic unity, moving through definite phases of development, they illustrate the fallacy of rhetorical reification or philosophical hypostasis. When they insist that culture is more than the sum of the traits which compose it they are not discussing the culture defined by Professor Tylor. With his definition we can do nothing else than equate culture with forms and artifacts and institutions.

The first methodological advantage of this interpretation of culture lies in the fact that it distinguishes man from culture.

Consistent and fruitful applications limit it to what men have produced as members of human society, some of which productions outlast the producer or inventor and endure from one generation to another. There is nothing superorganic nor even organic about this endurance. Cultural traits outlast man in the sense that a building outlasts its architect, a bridge its engineer, a poem its writer. All of these manifestations are a part of the complex whole of culture; they all exist external to man but do not transcend him; they constitute a complex whole to which he contributes and signify what he has as differentiated from what he is. A second, though problematic, advantage of this use of the culture concept consists in its potentiality as a criterion of progress. From this point of view "progress" has been defined as an increase in complexity of material and institutional traits, a definition which suffers from the fact that many primitive rites are as much beyond the comprehension of a member of an anthropological expedition as the elements of the differential and integral calculus are to the African Bushman in his native habitat. When progress is conceived in terms of increase in proper functioning, however, it is necessary to utilize culture traits, for they are the instruments of function. Further statements would require analysis of the concept of "progress" and exceed the limits of this paper. A third use of the culture which is a complex of traits involves a consideration of the term's extension—and we are immediately in the thick of the culture-area controversy. The problems concerned with the migration, circulation, diffusion, fading, modification, combination, invention, and dissociation of the components of culture lead directly to the conflict between the diffusionist and inventionist hypotheses as to the appearance of identical elements in various parts of the world. For our purpose it is important to emphasize that the culture area varies with the culture element in question, that there is no exact number of culture areas on the face of the globe, but areas for all the different articles of material and ideational traits, areas which, as Franz Boas has pointed out, depend to a large extent upon the subjective interest of the investigator.

We may conclude in general about this third type of culture

that its definition is clear and distinct, that its extent is more or less indefinite and depends upon the interest of the investigator quite as much as upon objective arrangement of data, and that it has three important uses: (1) to demarcate man from his own productions and creations, and to stand for what he has rather than for what he is (although, of course they constantly condition each other from the moment of birth); (2) to serve as a criterion of progress, although this use is as problematic as the meaning of the latter term; and (3) to provide grounds for analysis of the geographical distribution of traits thus to discover facts concerning their invention and diffusion.

The fourth and last type of culture is probably the most widely used and useful of all, and yet the most ill-defined and irregular in extent. Unprobed metaphysical hypotheses tincture the great majority of definitions of it. For Hegel culture is the general principle which manifests itself and becomes an object of consciousness in the state. For various philosophers of *Kultur* it signifies the bridge between matter and spirit, the genius of a people as it rises to its apogee in high forms of organization and accomplishment, or the dynamic unity and totality of social groups which develop according to discoverable laws, and which move in definite directions. Not only metaphysicians, but sociologists and anthropologists too use the word in this sense. Whenever they state that culture is dynamic rather than static they are discussing something different from social heritage. Whenever they talk about ideas asserting themselves in various cultures, whenever they refer to Chinese, Kwakiutl, or Western culture in phases of development and decay, and whenever they discuss the laws of cultural change, they refer not to a mere complex of traits, but to an undefined something to which processes can be ascribed.

To arrive at a just and accurate definition of this type of culture let us begin with Wilson D. Wallis's statement that "A culture is more than the sum of the things which compose it."[6] By asking a question corresponding to this remark we outline

[6] Wallis, *Culture and Progress*, p. 33. See identical assertion in Ruth Benedict, *Patterns of Culture*. Boston: Houghton, Mifflin, 1934, p. 47.

the problem: In what terms can the "more than the sum," the residuum, be expressed if we abstract all the things which compose it? The answer requires a new definition, the inclusion of an element covertly involved in all the discussions of recent literature, although never overtly recognized. The something more, the functioning dynamic unit, is the people who possess a certain complex of traits, including knowledge, belief, art, morals, law, custom, and the rest. The significant factor in every instance, the nucleus around which these traits are grouped, is the people who have them. The social group is the culture, the artifacts and traits are its attributes. Ordinarily the two senses are so interchanged as to result in the hopeless confusion apparent in current expositions. Defined and recognized as a social group, it becomes easy to show that the term culture assumes a significance which not only adds much to its methodological value, but also allows consistent application to all sociological and anthropological treatments of social change. In this sense, then, to avoid such metaphysical intrusions as national genius, dynamic ethos, collective spirit, social sensorium, and other ineffable and non-existent entities, a culture may be defined as a group of people living together and sharing common knowledge, customs, beliefs, material goods, institutions, rites and ceremonies—everything in fact which they produce and create as members of society.

This interpretation of the culture concept clarifies certain difficulties concerning sub-concepts. The compound term "culture-lag" can not possibly mean that one artifact or custom lags behind another. It is its adoption by human beings that lags behind. Only when people do not begin to use the second of two given cultural forms as early as they do the first, invented or produced or diffused contemporaneously with it, or relinquish the use of one sooner than the other, can the phenomenon of culture-lag occur. Similarly, culture conflict is not conflict between complex wholes of artifacts and institutions, but conflict between peoples—a fact which validates emphases upon the psychological foundations of culture. Gardner and Lois Murphy state, for instance, that when a new trait does not readily become assim-

ilated by an Indian tribe into which it has been introduced, it is not the culture pattern which resists it, nor the complex of culture traits already functioning, but the Indians themselves.[6] Any culture conflict situation reduces itself, upon analysis, to a disturbance in a previous status of function which culture traits have had with people. People produce or create traits; traits may influence people, but traits can never influence traits apart from people. Whenever one culture exerts an influence upon another it is people who are exerting or receiving the influence. They are the dynamic factor in the situation. Thus, every instance of culture conflict is simply a special case of a wider social interaction. Only when the use of "culture" emphasizes the people who share common attributes can the description of these active phenomena have meaning.

What are some of the methodological advantages of this interpretation of the culture concept? In the first place, these cultures are definite configurations which respond to the so-called laws of social change studied by sociologists. The application of laws or trends of social evolution to humanity or society en masse is apt either to be faulty or valueless because too inclusive, while the term social group is not only awkward, but often not inclusive enough. Cultures, whether spatial or temporal, tend to convey a sense of distinctiveness, while social groups themselves may often be small enough to duplicate each other. A culture is simply a member of the class, social group. As long as sociology, anthropology, and other social science disciplines are vitally concerned with the analysis of social groups, the culture concept can hardly be disregarded as a fruitful and potentially accurate means of objectifying ascertainable units for examination. Cultures in this sense are configurations of people and traits, the investigation of whose origin, growth, decline, and decay becomes meaningful. A second, related advantage is the consideration by which cultures become schemes of reference by means of which multifarious social data can effectively be arranged and utilized, and for which such words as "society" or

[6] Gardner and Lois Barclay Murphy, *Experimental Social Psychology*. New York: Harper and Brothers, 1931, p. 14.

"civilization" are useless because too comprehensive. Cultures in this sense too provide relevant contexts in which other single social and psychological phenomena can be related to each other and by means of which they can better be interpreted. They are schemes of reference, objectifications of social data for facile handling with relation to other groups of data and also for comparisons between the larger wholes, the cultures themselves. Another single advantage of this meaning of culture lies in the fact that it obviates the metaphorical use of such terms as "organism," "mechanism," "soul," and "spirit" as applied to human society. These analogies are so often abused as to reduce their efficacy to a minus quantity. As metaphors the terms may possibly be helpful even though logically inadequate; as definitions they distort and discolor the facts For societies are not organisms nor mechanisms nor spiritual entities—they are cultures. Few will deny the advantage of dispensing with superficial and often untrue analogies drawn from the "languages" of biology or physics and using instead terms like culture, germane to the universe of discourse of society itself. The difficulties of intertranslation of terms from two universes of discourse apparent in disputes of long philosophical standing are equally manifest in attempts to discuss society and social change in a terminology more appropriate to biology, physics, and religion. To use terms drawn from social scientific contexts belonging to the study of human society is to advance remarkably toward the goal of clarity. This may well be the most significant methodological advantage of the culture concept.

The emphasis upon the dynamic activities of human beings rather than upon static complexes of traits brings the psychological foundations of culture into their proper light. We need now only state the relation of geography to this fourth and final interpretation. The consensus of opinion among social scientists seems to indicate that while the regional or culture area approach is significant and necessary in the sudy of primitive societies where little social stratification is present and where communication is limited to primary groups, it can have no such methodological value when applied to modern highly complex and exces-

sively stratified society. This distinction becomes relative when it is recognized that cultures are groups of people sharing a common body of attributes. As Boas and others have maintained, for both primitive and modern groups traits assume priority over geographical regions. The assertion that most culture traits are limited to definite regions is really an elliptical statement to the effect that most culture traits are limited to definite groups of people, and the people are limited to regions. Whether or not cultures have geographical boundaries then becomes a secondary problem. Whatever case can be made out for geography depends to a large extent upon the fact that people who do the same things in the same way tend to live together in definite regions. Certainly cultures can not be defined by geographical criteria alone. The commonalty of a people's attributes and traits must always be the prime consideration.

The fourth meaning of culture, in summary, can clearly and distinctly be defined as a group of people, sharing common knowledge, belief, art, morals, law, customs, mores, institutions, and any other attributes invented, produced, or acquired by man as a member of society. Secondly, its extension varies with the size of the group which possesses the attributes singled out for investigation. In the third place, concerning utility, (1) because the culture concept helps to objectify social data for facile treatment, (2) because it provides a relevant context in which these data can be related, (3) because it describes a configuration which responds to whatever laws or trends of social change can be discovered, (4) because as part of the universe of discourse of society it obviates the use of superficial analogies drawn from other "languages," and finally (5) because it helps more clearly to delineate the problems involved in the study of human society, it may be considered a fruitful and valid concept for the social sciences.

We can conclude then with four interpretations of culture, all bearing some relationship to each other, but under closer scrutiny found to have fundamental distinctions and differences. The new definition of the fourth type of culture differs, too, from those most commonly contributed by philosophers of culture and of

history. With them it may be anything from a bridge between matter and spirit to the organic soul or spirit of a civilization, as pointed out above. In this paper it is a group of people sharing common attributes and traits. The significance of this terminological transition lies in the possibility now of discussing a dynamic and developmental culture, something apart from social heritage, in other than metaphysical terms. The idealistic lucubrations of Hegel and his followers have no place in a constructive sociology, nor in a positivistic philosophy of history. If cultures develop, if they grow, mature, decline, and disintegrate under a burst of irruptive power, or die of their own inanition, it is because these changes affect peoples and traits simultaneously in functional interrelationship, and not because some historical destiny idea manifests itself in successive stages of a triadic dialectic until, wearied, it is finally smothered in the Absolute. To define cultures in a positivistic way is to discover a vocabulary by means of which social change can fruitfully be analyzed, growth can be examined scientifically, and processes and functions and activities can be understood in something more—and something less—than metaphysical jargon. Although social scientists themselves painstakingly state that culture is to be taken in a dynamic and functional sense rather than as a static collection of traits, they continue to reiterate the Tylor definition. Tylor himself remained consistent, for his conception did not include growth and development. His followers recognize the new dynamic character of cultures without locating specifically the dynamic element, the element which this paper assigns to the people about whom a homogeneous collection of traits is clustered. Culture in the third sense, as social heritage, means in Ruth Benedict's felicitous phrase, "what binds men together"; a culture in the fourth sense, as the prominent concept in theories of social change, means men bound together.

All of the four interpretations of culture exhibited here present valid excuses for being. Three of them at least possess definite methodological value for the study of society and social groups. To conclude therefore that the term may not be used in all of these senses would require a judgment of value as to which was

correct, or the most nearly correct. This judgment need not be executed. Culture as personal refinement retains a legitimate place in discussions of manners and morals; culture as the higher accomplishments of group life maintains, as before, its meaning in histories of science and learning. There is further no reason why the word cannot be used in both of its more specifically sociological connotations if it be clearly recognized that in one sense it refers to the social heritage which binds men together, and in the other to men bound together by social heritage. The difference is that between traits and expressions and patterns of behavior as culture themselves, and as manifestations by which a culture can be delineated. It is merely necessary to appreciate the distinction in order that the methodological value which accrues to both interpretations may not be jeopardized.

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Mr. Miller's Interpretation of Whitehead

BY

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THE article by Mr. David L. Miller, called 'Purpose, Design and Physical Relativity', in the July 1936 issue of *Philosophy of Science*, consists mainly of a commentary on the development of Whitehead's thought from the *Enquiry Concerning the Principles of Natural Knowledge* to *Process and Reality*. Now most criticisms of Whitehead so far published have been from realists of the anti-metaphysical Cambridge school, complaining that his later works retract the teaching of his earlier works on the philosophy of natural science; and this complaint is raised quite unjustly, because a careful reading of the earlier works will show that there is nothing in them that precludes the logical possibility of a metaphysics of nature such as Whitehead expounded later. We should be grateful for any comment on Whitehead which does not repeat this hoary error. But unfortunately two opposed misinterpretations do not together make a correct interpretation. The opposite distortion of Whitehead is effected by those readers who, having found that the earlier books showed genius but were, alas, too much under the influence of ill-advised realist dogma, proceed to construe the succession of later books as the gradual self-correction of the great man towards a mature judgment which is in full accord with the reader's own. Of this distortion, I am afraid, Mr. Miller has given us an example.

His nominal text is M. H. Moore's short, general defence of the idea of 'a metaphysics of design without purpose', which appeared in the January 1936 issue of this Journal. Mr. Moore's thesis was that metaphysics should not inject the concept of the knowing mind, nor that of teleology, into its study of the problem of order or design in nature. Accordingly, Mr. Miller's first few pages appeal to Einstein's and Eddington's interpretations of relativity theory as showing that 'order is furnished by the mind'. This is a familiar story. Whitehead, however, was once so unregenerate as to present elaborate constructions of points, instants, time-systems and permanent spaces, conceptions of 'events' and 'material objects', theories of motion and of congruence, and a special and a general theory of relativity,—without once appealing to 'mind'. But when in later works he passed from these studies concerning the logical foundations of physics to a complete metaphysics of existence, and properly enough gave a central role to the 'prehending subject' (which is as close to 'mind' as one could expect a former realist to come) with its teleology, the obvious conclusion for Mr. Miller was that Whitehead had been driven to acknowledge the earlier work as 'weak', 'inadequate', even 'wrong'.

This conclusion is evidently gratuitous. The bizarre picture of Whitehead's work which it gives is best removed by briefly reviewing the diverse fields of Whitehead's philosophic activity. The length to which such a review will extend this communication is, I believe, necessary in view of the prevalence of mistaken ideas on the subject.

There have been many books dealing with that nebulous topic, philosophy of science. None, however, have defined their subject-matter as clearly as Whitehead did in his great *Enquiry*.[1] Most of the philosophical criticisms leveled at it appear irrelevant when we realize that its object was simply the full statement of what is touched upon in the first few pages of every book of theoretical physics:—precise, empirical, definitions of the meanings

[1] Following Mr. Miller's example, I shall use the first words of their titles as abbreviations for Whitehead's *Enquiry Concerning the Principles of Natural Knowledge*, *Concept of Nature*, *Principle of Relativity*, *Science and the Modern World*, and *Process and Reality*.

of the fundamental notions in terms of which the whole science is expressed, and of no other notions. The book is an attempt to remove, by means of a unified, comprehensive technical theory, the confusion which was then (1919) universal in our conception of 'the ultimate data of science'.[2] An example of this confusion was the conjunction of universal nominal allegiance to the relational theories of space and time, with the absence of any attempt to define points and instants relationally in a way that would justify their use in every statement of exact empirical science. Also, the theory of relativity had produced many highly refined statements of 'what we mean by space and time', but Whitehead believed that these scientific concepts 'are the first outcome of the simplest generalisations from experience, and that they are not to be looked for at the tail end of a welter of differential equations';[3] he purposed to examine the terminus of what is in his view a special type of experience, namely, sense-perception, with a view to finding these generalizations and giving them precise expression. In doing so, he had to define the concepts of 'motion' and 'congruence'. At the same time, he had to provide an alternative to the belief that the ultimate facts for science are given by configurations of material (matter, ether, or electricity) throughout space at a durationless instant of time. Thus Whitehead's topic was 'foundations'. This is a philosophical topic as well as a scientific one; but, as Whitehead remarked in the first chapter of his *Principle of Relativity*, 'philosophy' in this connection is not metaphysics; 'it should be called pan-physics. Its task is to formulate those principles of science which are employed equally in every branch of natural science.'

The *Enquiry* and its companion books, *The Concept of Nature* and *The Principle of Relativity*, are full of warnings to philosophers not to interpret pan-physics as metaphysics. For example, Whitehead says—as Mr. Miller, like Whitehead's antimetaphysical critics, does not fail to remind us—that for the purposes of his investigation, 'nature is closed to mind'.[4] But these critics

[2] A phrase from the Preface to *Enquiry*.
[3] *Enquiry*, Preface.
[4] *Concept*, p. 4.

do not quote Whitehead's next sentence: 'This closure of nature does not carry with it any metaphysical doctrine of the disjunction of nature and mind'. And concerning 'the values of nature', whose introduction in *Science and the Modern World* shocked so many English realists, Whitehead wrote, 'The values of nature are perhaps the key to the metaphysical synthesis of existence. But such a synthesis is exactly what I am not attempting.'[5] The exclusion of the closely related idea of teleology does not mean, as Mr. Miller supposes, that 'the external world has little if any teleology within it',[6] but only that teleology does not fall within the limited subject Whitehead was discussing.

Here is another warning that should be heeded by every philosopher who finds Whitehead's later works, as Mr. Miller has them, 'definitely acknowledging the difficulties' of the earlier works:— 'It is difficult for a philosopher to realise that anyone really is confining his discussion within the limits that I have set before you. The boundary is set up just where he is beginning to get excited.'[7]

Perhaps the critic's excitement may be sobered if he recalls that Whitehead's first published thoughts on some of the topics of these books were written in the symbolism of *Principia Mathematica*. For example, his celebrated idea of defining points in terms of what would ordinarily be called more complex entities had its first expression in a piece of pure mathematics, the 'theory of interpoints'. The memoir of which this theory was a part was called 'On Mathematical Concepts of the Material World',[8] and its standpoint was not even that of pan-physics, but purely mathematical or logical. Those who read all of Whitehead's works on 'the fundamental principles of natural science' in terms of the standpoint from which this topic is discussed in *Science and the Modern World* will find it hard to complain of the 'inadequacy' of a piece of work which does not pretend to be more than a logician's adventure in defining the

[5] *Concept*, p. 5.
[6] Miller, p. 268.
[7] *Concept*, p. 48.
[8] *Trans. Royal Society*, 1906, (Series A).

undefined elements of special deductive sciences in some wider scheme. They should also recall the paper, 'La Théorie Relationiste de l'Espace', which Whitehead read before a congress of mathematicians at Paris in 1914, and notice that its point of view was, roughly, half way between that of the earlier memoir and the later *Enquiry*. After they have observed this transition, which the *Enquiry* completed, from mathematics to pan-physics, they will be slow to assert, with Mr. Miller, that Whitehead's introduction of new ideas in the transition which then followed, from pan-physics to metaphysics, proves that it was a mistake to omit these ideas from pan-physics. We can only say that we are dealing with a mind interested in many things.

Of course, if an author has announced a certain standpoint for a particular purpose in a particular book, he must stick to that standpoint in that book; but Mr. Miller makes no complaint against Whitehead on this score. It is also legitimate to argue that the author's standpoint was inadequate for his purpose; but Mr. Miller's objection to Whitehead's standpoint in the *Enquiry* mistakes that book's purpose and substitutes instead that of later books. There was a straightforward line which Mr. Miller could have taken in answer to Mr. Moore. He could have claimed Whitehead's authority in arguing that the idea of teleology is needed by metaphysics in its study of the problem of order —which is precisely what Mr. Moore denied. But he wishes instead to appeal to that fashionable authority, 'scientific findings'.[9] Even this is not enough: he must show that a broad-minded man who pursued the limited scientific purpose has recognized that the idea of teleology was needed for that purpose. It is to be remembered that he treats not of the place of teleology in the science of biology, but in the theory of space and time.

The popularity of *Science and the Modern World* makes such mistakes easier than they should be. People start with the belief that this book presents 'Whitehead's philosophy of science', and then assume that it supersedes other writings in which Whitehead dealt with anything that might be called 'philosophy of

[9] Miller, p. 273.

science'. The use of a vague phrase thus produces distortion in criticism. This result may be avoided by observing that nowhere in *Science* are to be found statements of restrictions like those sprinkled liberally through the earlier books,—statements some of which I have quoted. The topic is no longer, as in panphysics, the principles employed by science, without the formulation of which scientists do not quite know what they are talking about. For example, without a clear definition of 'point' in terms of 'events', the scientist does not entirely know what he is talking about in his statements referring to space and time. The scientist does not, however, require that 'point' be defined in terms of 'prehensions of multiplicities ("data") into unities'. That is no part of his reasoning.

Some critics may be confused by the fact that both Whitehead's earlier books and his later books attack 'materialism' as a philosophy of science. This confusion may be avoided by contrasting the first chapter of the *Enquiry* with any chapter in *Science*.

This is all very simple; yet Whitehead's critics persistently forget it. The pathetic aspect of the situation lies in the pains Whitehead has taken, in all his works, to state the limits of his discussion. However, exaggerated attention to such statements also has its dangers. I am sure that some of those who do not make the anti-metaphysical critic's error of approving of the earlier works but finding them repudiated by the later works, nor yet the converse error committed by Mr. Miller, will fall into a third error by replying to these critics that the later works have *nothing* to do with the earlier. A passage in Section II of the first chapter of *Process and Reality* may help us to avoid this mistake. Whitehead is laying down the conditions for the success of imaginative generalization; and he remarks that a metaphysical construction must have its origin in a generalization started from some special field of experience, for example, from physics. *Science* presents Whitehead's generalization from physics. Its predecessors appear in retrospect as putting the realm of physics in order before generalization from it begins. Naturally, the organization of ideas earlier produced by abstract, special

theory is projected into the larger work; and to the fundamental ideas a new interpretation is given which makes them part of the exposition of human experience as a whole. Thus the earlier idea of 'process', which had meant simply 'the passage of events, within which spatial and temporal extension can be discriminated', becomes enriched by considerations of a sort that were barred for the earlier special purpose, and so transformed into the idea of 'prehension into unity'. Once such a step is taken, no sharp line can be drawn separating *Science's* 'philosophy of science' from Philosophy as a grand, inclusive subject.

But this step is *not* made, as Mr. Miller would have it, by meditating on pan-physics and amending that work. To describe the step adequately, we should have to expound the sub-steps involved, which is impossible here. The briefest hint that can be given to the reader, is to ask him to contrast the use Whitehead makes of a passage from Berkeley (Section 10 of the Fourth Dialogue in *Alciphron*) in his *Enquiry* with the use he makes of the same passage in Chapter IV of *Science*. In the former instance, Whitehead concludes with a statement of the *relatedness* of events requisite for understanding the scientific concepts of space and time; in the latter, his conclusion concerns 'what constitutes the *realisation* of natural entities'.

My interpretation of Whitehead's books should not be construed as implying something Whitehead has consistently deplored, namely, a 'divorce between science and philosophy', under the guise of a rough distinction between setting science aright and generalizing from science. The correction of foundations is essential for the clear statement of science. Generalization from science, as in *Science and the Modern World*, is essential for the scientist insofar as he is interested in the place of science in experience, and insofar as he recognizes breadth of thought as an ultimate ideal,—which is to say that such generalization is useful in the advance of science. But it remains true that scientific statements analyze restricted aspects of the complete fact described by metaphysics. This division appears in *Process* in two prominent forms: in the allocation of scientific observation to perception in the mode of 'presentational immediacy', as opposed

to the mode of 'causal efficacy' which alone is metaphysically fundamental; and in the distinction between 'genetic division', or division of the 'concrescence' (the process of prehension into unity), or the 'theory of feelings', and 'co-ordinate division', or division of the 'concrete' (the product of concrescence). 'Co-ordinate division' defines the subject-matter, in final metaphysical understanding, of the 'theory of extension'. It is impossible to explain the terminology of either of these distinctions at adequate length here; but a little reading of Whitehead will show that they correspond to the difference between the things dealt with in the predecessors and in the successors of *Science*.

Mr. Miller begins his own version of Whitehead's work by identifying[10] Whitehead's special, methodological closure of nature to mind and to metaphysics with Mr. Moore's position that teleology is no part of the order of nature for metaphysics.[11] He then quotes passages from *Science* and *Process* about the envisagement of eternal objects and the function of God to show that Whitehead has come to see the error of his ways. Of course Whitehead has arrived at a description of the teleology of natural processes, while preserving distinctions of standpoint obliterated by Mr. Miller. But Mr. Miller thinks[12] Whitehead's reasons on behalf of teleology are not clear. He believes that the compelling reasons can be found by considering the picture of the stratification of nature into a multiplicity of time-systems which the *Enquiry* and its companion-volumes produced. This stratification provided a basis for the facts of relativity. Mr. Miller's reflections upon it form the bulk of his article. Accordingly, he presents his comment on Whitehead as an argument showing how relativity leads us away from the closure of nature to mind, to-

[10] Miller, p. 272.

[11] To avoid misleading readers of Mr. Moore's article, I add the observation that his position shows more of compatibility than of incompatibility with Whitehead's. This is because both are realists, in the sense in which Mr. Moore speaks of realism. And the only teleology which Mr. Moore discusses is an 'extranatural' teleology, arbitrarily and externally operating on the system of nature. Any notion of that type would of course be rejected by Whitehead as well; in fact, such notions are hardly worth discussing. The important, Whiteheadian arguments for teleology are thus not touched by Mr. Moore.

[12] Miller, p. 273.

ward a nature replete with mental purpose. This is what links the comment on Whitehead with the briefer discussions of Einstein and Eddington. The old idea of making relativity theory campaign for idealism shows a new boldness when, faced with the complexities of Whiteheadian realism, Mr. Miller roundly declares that[13] 'the problems around which his (Whitehead's) philosophy of organism is focused have arisen for the most part out of an attempt to reconcile his realistic position in philosophy with the theory of physical relativity.'

I challenge Mr. Miller to find in all of Whitehead's writings a single passage that will serve as partial evidence for this statement. The minimum requirement for the present topic should of course be one passage showing that the theory of relativity requires or at least suggests something about teleology. Of course one can find passages discussing relativity in the light of the philosophy of organism, which involves teleology; but their purpose is only to show that a niche can be found in the general philosophy for the accepted facts, and for Whitehead's own scientific theory, of relativity; they do no more than describe the niche in terms of the architecture of the philosophic building. In Whitehead's view,[14] the same thing can be done, in theory, for any type of notion. (But *relativity* has a fashionable authority!) for example, Whitehead finds a place for logical notions in *Process's* scheme of philosophic notions; yet one could hardly interpret this as correcting particular work in the interrelations of logical notions, such as Whitehead produced thirty years ago.

If Mr. Miller were to confine himself to insisting that the idea of a 'prehending subject' must play a rôle in our final—that is to say, in our metaphysical—understanding of the extensive continuum, then as a disciple of Whitehead I would agree with him, remarking only that we should prefer Whitehead's actual construction of this rôle in his theory of 'strains'[15] (to which Mr. Miller nowhere refers) to Mr. Miller's argument that there must be a rôle. But since Whitehead's ideas on teleology, of which we

[13] Miller, p. 272.
[14] See Chap. I of *Process*, Part I.
[15] See Part IV of *Process*.

both approve, are unpopular and yet fundamental, they need to be discussed with more care than Mr. Miller has exercised. Therefore I am asking him for evidence from Whitehead's writings showing that relativity theory exerts some compulsion, in however slight a degree, upon us to accept teleology as a fundamental natural category either for science or for metaphysics. It is easy to find instances of the lack of such influence. Thus, Mr. Miller spends two pages appealing to the teleology inherent in the 'epochal theory of time', one of Whitehead's central doctrines. But we have Whitehead's statement that 'It is to be noted that this doctrine of the epochal character of time does not depend on the modern doctrine of relativity, and holds equally—and indeed, more simply—if this doctrine be abandoned.[16]

The fifteen pages of reasons which Mr. Miller gives to show that Whitehead's relativistic exposition of space-time systems in the *Enquiry* leads us inexorably to the teleology of *Process* are, I make bold to say, entirely his own conjectures, involving the obliteration of a distinction between types of analysis of fact, which Whitehead retains. The fact is that Whitehead's arguments for teleology are often stated, abundantly clear in intent, and of an entirely different type from those Mr. Miller would have logic fasten upon Whitehead. They take the form of an appeal to the analysis of direct experience of all sorts: religious, social, poetic, commonplace. I will recall only the chronologically first instance of such analysis. In Chapter IV of *Science*, Whitehead studies simple sense-awareness, but without the restrictions under which that topic was studied in his *Enquiry*; he arrives at the conception of nature as a complex of prehensions. In the next chapter, he develops the idea of prehension by passing from simple sense-awareness to the analysis of direct experience of all sorts; and it is here that the idea of teleology is introduced to enrich the bare idea of prehension.

Whitehead may at times have sat down and thought along Mr. Miller's lines instead; but there can be no doubt that he has often and forcibly argued that cogent philosophic method proceeds

[16] *Science*, p. 185.

by analyzing concrete experience and not by analyzing refined theory constructed for scientific purposes. Thus Mr. Miller's philosophizing runs exactly counter to the great rule for philosophizing which, it might be argued, is of all Whitehead's doctrines his most valuable contribution to philosophy.

As I review Mr. Miller's assertions about Whitehead's supposed change of heart, it seems to me that it should be possible to quote these assertions in order and show a mistake in each. Limitations of space prevent this, of course. I will be content to point out by a few examples the influence of what I believe to be the main source of these errors—Mr. Miller's dissatisfaction with the realistic and methodological exclusion of mind from Whitehead's earlier works. Consider the very first sentence of his detailed discussion of Whitehead.[17] 'Although we have [in discussing Einstein and Eddington] brought out the problem of knowing a design independently of mind, there yet remains the problem of showing whether or not there is a design in nature which is free from teleology or from the influence of conscious ends.' Now Whitehead's metaphysics is, in one of its functions, a description of 'the teleology of nature', as Whitehead says. But 'the influence of conscious ends' in determining design in nature is something one will not find described or defended anywhere in Whitehead's writings; on the contrary, one of the first things the student learns about Whitehead's philosophy is the unimportance he assigns to the conscious as a basic metaphysical concept. Since the remaining thirteen pages of Mr. Miller's article are devoted entirely to Whitehead, it would appear that the only function effected by the coupling of two notions which Whitehead carefully disengages is to further the confusion of what Whitehead says with what a philosopher approving of the traditional rôle of 'mind' would like to have him say.

The first description of Whitehead's early works which the reader comes upon in Mr. Miller's article shows the same distortion. Whitehead's works on natural knowledge, Mr. Miller says,[18] were 'designed to explain how it is possible to have a

[17] Miller, p. 272.
[18] Miller, p. 268.

knowledge of "nature closed to mind". By "closed to mind" Whitehead meant "unaffected by the observer". According to these earlier works the external world had little if any teleology within it . . . .' The first of these sentences is a mistake because discussion of the possibility, the philosophical 'how' and 'why' of natural knowledge, is precisely what Whitehead in these books excluded in favor of description of its 'what'.[19] The second statement mistakes the meaning of the doctrine that nature is closed to mind. That closure meant simply that 'none of our perplexities as to Nature will be solved by having recourse to the consideration that there is a mind knowing it.'[20] Mr. Miller's third sentence is wrong because neither the *Enquiry* nor its companion volumes say anything for or against teleology in nature.

The reader will have no difficulty in finding other mistaken constructions following upon these. I quote one which is rather subtle: Mr. Miller's interpretation of 'prehension' as 'uncognitive apprehension'.[21] This interpretation has its formal justification in one of Whitehead's endeavors to make an audience understand the idea of 'prehension'. He said in effect,[22] 'Think of "apprehension", and think away as unessential the cognitive character which we associate with the word.' By choosing a description which allows him to define prehension as a species of apprehension, Mr. Miller gains an advantage which would not be his were he to refer instead to some of Whitehead's other explanations of the meaning of 'prehension'. For example, 'blind physical perceptivity'[23] might introduce a discordant note into the uncritical assimilation of Whiteheadianism to idealism.

In fairness, we ought to remark the extreme difficulty of commenting without error on the complexities of Whitehead's work. And I should not condemn, but welcome, the idea of expounding how the same extensive continuum which Whitehead abstractly

[19] See *Enquiry*, Preface. On p. 3 of this Discussion I have tried to explain what 'our perplexities' were.

[20] See *Concept*, pp. 28, 32.

[21] Miller, p. 284.

[22] *Science*, p. 101.

[23] See the paper on 'Time', read before the Sixth International Congress of Philosophy, Harvard, 1926.

described in his earlier works provides, in his later works, the generic morphology for a cosmos involving teleology in every concrete detail. My criticism of Mr. Miller's article has of course nothing to do with the acceptance or rejection of teleology as Whitehead has defended it. It has to do with his distortion of Whitehead's work, and with the question of philosophic method, —a question whose importance transcends the problem of interpreting Whitehead. My criticism of Mr. Miller's method applies also to his early pages, wherein he appeals to Einstein's and Eddington's authority in behalf of the Kantian doctrine that spatio-temporal order is furnished by the mind. My objection is, that appeal to refined scientific theory is too deceivingly easy to make, and too easy for an opponent to destroy. Thus a scientist who reads Mr. Miller's article will quickly remark that he can go through Eddington's *Mathematical Theory of Relativity* and remove the occasional references to 'mind', without having lost anything which concerns science; and that 'mind' is not mentioned in Einstein's *classical paper* on the Foundations of General Relativity Theory, to which Mr. Miller makes the mistake of referring when claiming Einstein's support.

The bulk of Mr. Miller's paper is intended to show that an eminent thinker who was so naive as to attempt to expound the event-structure without introducing mind, was compelled by scientific findings to introduce a prehending subject with its teleology. But the eminent thinker did not introduce teleology into his philosophy by appealing to scientific findings; and it is foolish to try to make him do so, if one has the idea of teleology at heart. For nothing can discredit this idea more than appearing to draw it out of the hat of relativity theory.

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REVIEWS AND NOTES

Determinismus und Indeterminismus in der Modernen Physik. Ernst Cassirer. Göteborg: Wettergren & Kerbers Förlag. 1937. Pp. ix + 265. 8 Kr.

This attractively written book brings to bear upon current discussions of causality Professor Cassirer's great historical erudition as well as the insights characteristic of the Marburg Neo-Kantian movement. The point of view advanced in the present study will be familiar to readers of Cassirer's earlier writings, especially his *Substance and Function.* But if nothing essentially new emerges, and if one can legitimately ask for a somewhat more precise and detailed handling of some of the issues involved, the book has many illuminating things to say on the status of the causal principle, and its sets a standard of maturity which few physicists themselves have attained in their *obiter dicta* on it. The book also shows that on many important points it is only his Kantian terminology and Kantian piety which distinguishes Cassirer from many naturalistic pragmatists and logical empiricists who have concerned themselves with similar themes.

The book is divided into five parts, having the following titles: Historical Preliminaries, The Principle of Causality of Classical Physics Causality and Probability, The Problem of Causality of Quantum Theory, and Causality and Continuity. Its argument is briefly this: Most current discussions of the principle of causality have assumed that it makes statements about natural objects and their mode of interconnection. When interpreted in this way the principle of causality engenders an agnostic scepticism as to the possibility of knowledge, and makes it impossible to defend the principle against the onslaughts of modern physics. Cassirer has nothing to say in support of such a conception of the causal principle. He insists, on the contrary, that it does not formulate a law of nature: it is not on a par either with singular statements reporting the result of measurement; or with empirical laws asserting general connections between measurable magnitudes;

or with theoretical principles, such as that of least action, which bring together into a system such empirical laws. The principle of causality, according to Cassirer, is a regulative principle for the construction of the science of physics, and it requires physical inquiry to be so conducted that it aims at a *logical system* of physical knowledge. In brief, the principle of causality is a methodological principle concerning the *form* which our knowledge of the laws of nature is to take.

With the principle of causality so construed, it is not difficult for Cassirer to show that nothing in modern physics offers any ground for challenging it. He makes good use of the abstract character of quantum theory to support his view that for theoretical physics Nature is not a set of *objects* but a complex of *relations*, in which functional interconnections replace self-subsisting and inaccessible substances. Modern physics does not break with the principle of causality because its laws and principles have a different form than those which obtain in classical physics, and Cassirer shows that the logical form of probability statements makes it imperative to regard them as elements in just such a system of knowledge as is required by his interpretation of the causal principle. Not even the principle of indeterminacy requires the rejection of "strict laws" of nature; this principle simply indicates how such "strict" laws may be obtained: the relations asserted by physics must hold between *different physical states* than the states contemplated by classical physics. Consequently, the one thing quantum physics has definitively established is that it is impossible to attain that unified, systematic physical knowledge which is postulated by the principle of causality, if we insist upon operating in terms of the picturable models of the older physics. The crisis of modern physics, according to Cassirer, is not a crisis with respect to the principle of causality but with respect to the naïve demand that Nature (i.e., a complex of relations) be picturable or intuitable. Thus, while physical theories must be controlled by experiment and observation, the theories themselves are not matters to be pictured or intuited in a sensory way.

Since the principle of causality makes no assertion about matters of fact, Cassirer can maintain it is apriori and that it formulates an invariant of thought. It is clear enough that the principle cannot be "refuted" on grounds of sensory observation, but Cassirer's discussion does not make clear just what its status is. He does not seem to regard it as simply a methodological resolution, though perhaps his position is not incompatible with such a view; it does express an absolute intellectual invariant to which there seems to be no alternative, but on this

point the present book, like Cassirer's previous ones, has left the present reviewer a good deal in the dark. Kant's transcendental psychology, which plays no part in the rest of Cassirer's discussion, does seem to flit obscurely across the stage at this place. There is a concluding chapter to this study which examines the import of the principle of causality and quantum mechanics for ethics and the problem of human freedom; its very satisfactory outcome is that the import is nil.

ERNEST NAGEL.

Columbia University.

Einführung in die Mathematische Logik. Alfred Tarski. Wien: Julius Springer, 1937. Pp. x + 166. RM. 7.50.

Dr. Tarski, who is one of the most gifted of the younger generation of logicians, has written an admirably clear introduction to the concepts and problems of formal logic. Part I contains lucid accounts of the sentential calculus, the theory of classes, the functional calculus, and the main features of deductive methods. It includes accurate discussions of the variable, of definitions, of identity, of sentential functions, and of descriptions. Part II consists of the application of the logical notions developed in Part I to a postulational reconstruction of parts of arithmetic. Questions of the independence, consistency, and completeness of postulates are handled with great skill, and the fundamental importance of such notions as "group" and "field" is explained with amazing simplicity. All the topics Dr. Tarski touches upon are amply illustrated, most of the examples being taken from ordinary elementary algebra. Each of the ten chapters is followed by a series of graded exercises, which should prove to be a boon to teachers of the subject. One of the remarkable features of this little book is that all the fundamental notions of modern logic are conveyed to the reader without the use of special symbols; the only ideograms that occur in it are those employed in familiar mathematical theory. In spite of the fact that the book is intended for beginning students, it manages to convey to the reader the scope and direction of modern researches: even the work of Gödel is briefly explained. It goes without saying that like all of Tarski's writings, this book is formally "correct"; not the least of its merits is its insistance upon distinguishing between *using* a symbol and *speaking about* a symbol. Before the reader is through with it, he learns to appreciate the importance of precision and appropriateness of expression.

The one fault of Dr. Tarski's book is its brevity, and this reviewer

would have liked to have seen him present the conclusions of more advanced researches into formal methodology, and their bearing on current logical issues. The beginner would also have been grateful if references had been supplied to the literature where topics only briefly treated in the present volume are more fully discussed; the only references given are to books of an elementary character. Dr. Tarski is one of the few people who is capable of writing a systematic treatise on the methodology of the formal sciences. All students of the subject must wish, therefore, that with this first-rate elementary work out of the way, he will soon give us that much needed larger treatise.

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Time's Arrow in Society, by Anderson Woods. University of Chicago Press, pp. xi + 233, 1935. *Recent Theories of Sovereignty*, by Hyman Ezra Cohen, University of Chicago Press, pp. xi + 169, 1937.

Students of the contemporary world scene who find themselves blanching over the fare of aggression, destruction and homicide offered daily in the press might do well to look into Anderson Wood's study in optimism, *Time's Arrow in Society*. It may be of some small comfort to them to know that the course of time's arrow, according to Mr. Wood's carefully reasoned ethical treatise, is "inevitable progress."

Of course, Mr. Woods' viewpoint is a long-run one, plotting the direction of social movement not from the tangents of the curve of present-day affairs, but from the whole evolution of society. This evolution is seen to occur through the development of the component systems of society in successive—if overlapping—stages of conflict, reconciliation of antagonisms, harmony realized and, after each system has got so far, through ultimate harmonization of the "larger conflict systems." As for the day-to-day mechanics of the development, Mr. Woods relies apparently on some good old-fashioned hedonism to see him through. All individuals set out to maximize their pleasure and end by maximizing everybody's pleasure. Individual desires, falling into conflict systems, are "progressively harmonized and reconciled after the fashion of the equilibration going on throughout general nature." Both the conservative and radical, who see this universal process as already arrested or as ever inverted, err. In supposing that they can affect the course of time's arrow, both, in fact, are "fools."

While Mr. Woods' sequence of ideas is very straightforward, his

"philosophy of progress" seems basically to have been decided by the thought that society, having for a sufficiently great time interval undoubtedly advanced, must necessarily continue to do so over subsequent intervals.

Recent Theories of Sovereignty by Hyman Ezra Cohen is an intensive study of the ideas of Esmein, Jellinek, Duguit, Kelsen, Laski and other political scientists, conducted in the hopes that the future of the concept of sovereignty would thereby be revealed.

Following a practice not uncommon among scholars, Mr. Cohen adopts the expedient of replying to one question with a bushel of new questions. Basic to all the theories discussed, says Mr. Cohen, is "the age-old problem of the relation of man to society. How should man conduct himself in relation to the social environment which surrounds him? . . . how should society, how should the social environment, conduct itself in relation to man whom it encompasses? then we have the problem of sovereignty revealed. The problem of sovereignty is essentially and vitally that of how much control has society over the individual man? To what extent is he subordinated to external social compulsion?" *etc., etc.*

With such an imposing array of questions on his agenda, it is not surprising that Mr. Cohen arrives at a conclusion no more definitive than that "the theories of 'sovereignty' may disappear with changes in terminology; but the substance of sovereignty will remain so long as the problems of social control divide men into rulers and ruled, into leaders and led."

HARRY MALISOFF.

New York, N. Y.

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BOOKS RECEIVED

Beyond Humanism. Essays in the New Philosophy of Nature. By Charles Hartshorne. Willett, Clark & Co., Chicago-New York. 324 pp. $2.50.

Plato. Phaedrus, Ion, Gorgias, and Symposium, with passages from the Republic and Laws. Translated by Lane Cooper. Oxford University Press. 436 pp. $3.50.

Moritz Schlick Gesammelte Aufsaetze. 1926–1936. Gerold 7 Co., Wien.

Through Science to Philosophy. By Herbert Dingle. Oxford: At the Clarendon Press. 363 pp.

Experience and Prediction. By Hans Reichenbach. The University of Chicago Press. 410 pp. $4.00.

A Biological Approach to the Problem of Abnormal Behavior. By Milton Harrington. The Science Press Printing Co. 459 pp.

Consciousness Creative. By Francis J. Mott. A. A. Beauchamp, Boston. 395 pp. $5.00 postpaid.

First Account of Order. By Edward Branner, 590 Fort Washington Ave., New York City. Privately printed.